AMERICAN POLITICAL SCIENCE REVIEW

SEPTEMBER 1988
VOLUME 82 NUMBER 3

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In Memoriam

Harvey C. Mansfield, Sr.

Managing Editor
American Political Science Review
1956–1965

Died 4 May 1988

QUARTERLY JOURNAL OF THE
AMERICAN POLITICAL SCIENCE ASSOCIATION

SEPTEMBER 1988
VOLUME 82 NUMBER 3

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ARTICLES

CONSTITUENCY CANDIDATES VERSUS PARTIES IN JAPANESE VOTING BEHAVIOR

BRADLEY M. RICHARDSON
Ohio State University

Most studies of Japanese elections have seen constituency candidates and their campaigns as the major factor in electoral mobilization. In order to explore the effects of candidate campaigns and voters' images of candidates, as well as party identifications, party images, partisan issue opinions, and past voting, I develop models portraying these effects upon the vote, both separately and together, and test the models using survey data. The results show that partisan attitudes are the dominant force in Japanese parliamentary voting. The most plausible explanation for the importance of partisan factors lies in the electorate's being exposed regularly to information about parties but only intermittently to information about candidates. The plausibility of an information-based explanation of the importance of robust partisan attitudes in Japan suggests in turn that comparative differences in voting may reflect systemic variations in information processes.

Little is known about the relative influence of parties versus local constituency candidates in parliamentary elections. One reason constituency variables have been omitted in research on parliamentary election voting is that elections frequently seem to be dominated more by national parties than by local forces. Centralized party organizations and party list voting would both seem to encourage voter concern for partisan elements of choice. But parties and campaigns are more decentralized in some countries, and electoral laws may encourage a focus on constituency candidates. In these kinds of settings, constituency candidates and their campaigns may actually be very important in parliamentary election voting.

This is a study of partisan versus constituency candidate influences on voting behavior in a Japanese parliamentary election. Japan is a setting where local constituency campaigns and candidates are often seen as unusually significant. Using survey data on voting in a Japanese general election, I demonstrate that local candidate imagery, exposure to constituency campaigns, and the effects of vote solicitation within local social networks have a significant impact on the decisions of many Japanese voters. However, my findings also show conclusively that party identification, party images, and party-focused habitual voting are the main determinants of the vote. As a corollary to the importance of party variables, people have more developed images of parties than they do of candidates. The more consistent availability of information on parties and consequently greater cognitive buttressing of partisan attitudes appears to be a major factor underwriting the importance of party variables in Japan. The findings of this research qualify earlier ideas that constituency candidates and

AMERICAN POLITICAL SCIENCE REVIEW
VOL. 82 NO. 3 SEPTEMBER 1988

their campaigns dominate Japanese parliamentary voting.

Constituency Candidates in Voting Research

Early political psychology was more interested in the partisan dimensions of voting choice than in the effects of constituency campaigns or attitudes toward local candidates. U.S. political psychology from the beginning focused on presidential contests and saw attitudes toward presidential candidates, opinions on partisan issues, and party identification as the main influences on voting decisions (Campbell et al. 1960). Research within the psychological tradition on voting overseas also focused on attitudes toward parties and partisan issues (Baker, Dalton, and Hildebrandt 1981; Valen and Katz 1964). Even where constituency dimensions of the vote could potentially be important, as in Britain's single-member districts, parties and partisan issues have been emphasized in most voting studies (Butler and Stokes 1969; Crewe 1976; Crewe, Sarlvik, and Alt 1977).

In the 1960s U.S. research began to examine the effects on voting of attitudes toward constituency candidates in congressional elections. But knowledge of candidates proved to be low, according to their findings, and party identification still appeared to be the main determinant of choice (Stokes and Miller 1962). More recently, research based on new measurement strategies has disconfirmed the idea that U.S. congressional elections are solely party contests. Instead, evidence shows that local candidate evaluations are a prominent determinant of choice (Mann and Wolfinger 1980). As a result, attitudes toward candidates and other variables that tap exposure to constituency campaigns have come to occupy a more prominent position in congressional voting behavior analysis (Asher 1983). However, up until now, this research has had very little impact in research abroad. Just how things work out in the presence of different combinations of party-versus-constituency influences on voting in different systems remains to be ascertained.[1]

Constituency Forces in Japanese Elections

Institutions and Setting

Nowhere is the constituency candidate dimension of electoral choice so plausible an influence on voting as in Japan. Electoral institutions, typical campaign practices, and political culture all converge to make candidates and their local campaigns potentially very important factors in electoral behavior. Multimember districts predominate in elections for both houses of the national parliament; they are the exclusive pattern in House of Representatives elections and an important mode in House of Councilors local contests. Parliamentary seats are assigned on the basis of individual candidates' district vote totals in most cases. (Only in the House of Councilors' national district, which selects 100 of the upper house's 252 members, is proportional representation the rule.)[2] More than one candidate from the same party can consequently be elected to the Japanese system if a party's district vote pool is large enough. Responding to these circumstances, both of Japan's leading parties, the Liberal Democrats and the Japan Socialists, run multiple candidates in many constituencies. Multiple candidates are virtually the norm among Liberal Democrats; they are less common in the case of the Socialists. Under this set of circumstances, candidates from the same party compete with each other as well as with candidates from the other parties.

Candidate-focused Campaigns

Under a combination of multimember districts and intraparty competition, constituency election campaigns are often run mainly by the candidates themselves. The parties play a larger role in election strategy in the parties of the Middle and Left; but even there candidates account for much of the initiative in the constituency portion of the campaign. A number of factors besides the institutional setting and intraparty competition actually encourage candidate-focused campaigns and may have underwritten the historical development of Japan's unique electoral arrangements. These include the small areas, dense populations, redundant and accommodating social structures and supportive cultural norms that define the context of campaigns in many of Japan's parliamentary election districts (Richardson 1974). The presence of local elites who manipulate the vote, community norms and group processes that encourage conformity, cultural values that emphasize personal linkages over abstract ties, and social exchange practices dictating that gifts often be given for the favor of a vote all permit, and even encourage, candidate-focused politics in Japan. The social setting thus combines with the effects of electoral laws to support an unusual degree of emphasis on candidates and their personal connections.

Campaign strategies in Japan reflect the social and institutional parameters just described. Candidates, particularly those within the conservative movement, tend to emphasize their own records and positions more than party affiliations, according to Dore's (1955) analysis of formally registered election statements. Candidates also make strong appeals to local interests and endeavor in both the short and the long run to mobilize the vote in their own favor, for instance, by organizing support groups or gaining the endorsement of community influentials. Anticipating the "favor" of a vote by providing small services, token gifts, or even candidate-sponsored trips is also not uncommon (Curtis 1971; Richardson 1967). Candidate-oriented campaign techniques and appeals are reputedly most developed in rural districts and in small cities. But even in larger urban constituencies candidate-organized support bases and "traditional" campaigns are found more often than might be expected (Richardson 1979). Obviously candidates are a major feature of Japanese elections, which could make attitudes toward candidates and responses to their campaigns a major component of voting choice.

Constituency and Candidate in Japanese Research

Findings from several sources indicate that constituency considerations influence the vote in national elections in Japan. According to studies dating back to 1958, roughly two out of every five voters in House of Representatives elections normally cite constituency candidate as a more important factor in their vote than party. The focus on candidate has stayed remarkably stable over the past three decades (Table 1). (A concern for party has gained somewhat in this period, accompanied by a decline in *don't know* responses.)

Other evidence supports the theme that constituency candidacies and campaigns are important in Japanese national elections. Contacts with candidates in the form of postal greeting cards, membership in candidate support organizations, attending speeches by the candidate, and requests to vote for a particular candidate are widespread in Japanese parliamentary elections according to the findings of a Komei Senkyo Remmei-Akarui Senkyo Suishin Kyokai series of voting studies extending back over two decades (Richardson 1979). Candidate evaluations, measured by thermometer questions, are also

Table 1. Emphasis On Candidate or Party in Voting Choices in Japanese General Elections (%)

Year	Candidate	Party	Don't Know
1958	45	32	23
1960	43	33	24
1963	51	31	18
1967	47	37	16
1969	33	49	18
1972	38	48	14
1976	40	46	14
1979	46	41	13
1980	38	49	13
1983	42	47	11
1986	39	49	12

Note: Samples were nationwide; all sample numbers were over 2,000.

Source: 1958–67, *Sosenkyo no jittai,* a private election survey report series of the Komei Senkyo Remmei; 1969–76, *Shugiin giin sosenkyo no jittai,* a similar series of reports from the *remmei;* 1976–86, the Akarui Senkyo Suishin Kyokai, a successor organization to the *remmei.*

very important to the vote and overshadow the effects of vote requests and contacts with the candidate, according to some research (Watanuki 1986a, 1986b). Each element of constituency candidate-oriented voting—a response to candidates' personal qualities and reactions to efforts to mobilize the vote—constitute dimensions to be considered seriously in Japanese voting research.

Partisan Components of Japanese Electoral Choice

Evidence from various sources suggests that a "constituency campaign" model of voting behavior might go a long way toward explaining electoral choice in Japan. However, other evidence from research in Japan provides support for an alternative paradigm of the vote, wherein parties and partisan issue attitudes are the main determinants of choice.

Political Parties

Japan has had national political parties since the 1890s. Contemporary parties date back to prewar antecedents in the case of the Japan Socialists and the Liberal Democrats. Both parties experienced several mutations before assuming their contemporary form in 1955, and the postwar Socialists are a far more significant party than their prewar predecessors. Still, these two parties have been around for quite awhile in one or another form. The same is true for the Japanese Communist party, which came into being in 1945 and had been a consistent electoral alternative for three decades by 1976. Only the Democratic Socialists, who left the Japan Socialist party in 1959, the Clean Government party, which first appeared as a contestant in national elections in 1967, and the New Liberal club formed in 1976, were comparative newcomers to the political scene at the time of the 1976 study. My earlier research in Japan addressed the effects of discontinuities on partisanship (Richardson 1975), but with the passage of time the parties' continuity is a more prominent factor.

In addition to their relative longevity, Japan's national political parties are highly visible in daily political life. Even if the parties are not as active in many constituencies as the local candidates, all parties still conduct visible national campaigns, including, since 1969, closely regulated television presentations of party platforms. Election manifestos are announced and publicized, and party leaders make campaign speeches throughout the country. Interparty conflict in the Diet and party policy pronouncements between elections also receive heavy media coverage. The durability of interpartisan differences on foreign policy, as well as long-term divisions on some domestic issues imbued with class rhetoric by Japan's permanent opposition parties, gives special emphasis and clarity to party differences (Richardson and Flanagan 1984). Because of their permanence and high visibility, the Japanese electorate has ample opportunity to learn about parties and party differences on issues.

Party Variables in Voting Research

Parties and issues are clearly important to Japanese voting behavior according to many studies of electoral choice (e.g., Akuto 1971; Kohei 1972, 1974). Indeed, while party identification has been identified as weaker and less stable in Japan than in other countries (Flanagan and McDonald 1979; Miyake 1970, 1985, 1986a; Miyake, Kinoshita, and Aiba 1967), identifications have still been shown to play a major role in the electoral choices of Tokyo's voters (White 1984) and in the decisions of persons in districts where each party runs only one candidate (Rochon 1981). Other partisan components of a psychological model of voting choice, such as partisan issue attitudes, partisan ideology, and party imagery, are also clearly factors in electoral behavior in Japan (Kabashima 1986; Kohei 1979; Miyake 1986b).

Social Change and the Prominence of Party Variables

Recent rapid social change and advances by the electronic media in Japan appear to have enhanced the importance of a partisan components model of the vote. Four mutually reinforcing processes are involved in these changes: cities are growing in population, industries have invaded the countryside, television is widely diffused, and the past three decades have seen an explosion in the numbers of persons obtaining high school and college educations.

Urban constituencies have always been more the arena where interparty, national issue-focused competition had the greatest electoral resonance (Richardson 1966, 1974). The social networks and local elite influence that underwrite candidate-run campaigns in the countryside and small towns and cities are to some degree attenuated in the more anonymous society of the large cities. Simultaneously, broadly conceived partisan issues have been the main concern of persons in the cities, whose lives and occupations are cosmopolitan in so many ways. Urban districts are also the locale where the written mass communications media were first developed and diffused, a fact that combined with other forces, such as the national political orientation of Japanese labor unions, give cities a particularly cosmopolitan and partisan political life.

As the cities have expanded, so have urban styles of political behavior. Much recent urban growth has taken the direct form of expansion of the cities and their suburbs into former farming areas. This process, which was especially prominent in the late 1960s, led to a drastic change in the social composition and political color of many constituencies (Flanagan 1980; Richardson 1977). Elsewhere urban social and political forces have penetrated the countryside via regional industrial development. In addition, the spread of television has supplemented the already widespread diffusion of the highly centralized written mass media, so that the countryside is itself subject to more partisan information than was the case earlier. Motivating and responding to all of these events was a dramatic restructuring of educational opportunities and accomplishment, with the result that virtually all persons now finish high school, and college attendence has nearly reached North American levels—a big advance over the earlier postwar era (Richardson and Flanagan 1984). Each of these factors—urban residence, industrial occupations, mass media diffusion, and expansion of education—is associated with a preference for party over candidate (Akarui Senkyo Suishin Kyokai 1986).

Social change thus plausibly underwrites the growth over time in the importance of party shown in Table 1.

Party and Candidate in Political Communications Processes

The urbanization of Japanese life and the accompanying widespread diffusion of the written and electronic media are processes that have been underway for a very long time but accelerated beginning in the late 1960s. They take on special importance for this analysis when differences in the information processes concerning parties and candidates and their implications for attitude development are fully appreciated. Parties are the substance of the electronic and written communications media, which are widely diffused in Japan and deal with politics on a regular basis. In contrast, candidates are mainly the subject of informal, verbal communications, which for most people are intermittent. These differences in the regularity with which people are exposed to political information are extremely important.

The mass media are highly centralized in Japan and their news coverage deals mainly with national and cosmopolitan information. Most Japanese read only national newspapers, whose coverage of local affairs is especially scanty relative to their treatment of national and international news. Television coverage of politics and elections is likewise centrally administered and focuses on national and international politicians and events. Some of the public, somewhere around one-fourth of all newspaper readers, do read regional newspapers where the news fare is somewhat more local in orientation (Meideia Risachi Senta 1986). Local pages that carry news for each prefecture (and which are different for each prefecture) are also found in all of the centralized dailies. But unlike the United States, neither prefectural newspapers nor local pages in the national dailies provide much coverage of individual candidates' campaigns. This reflects either the small size of the relevant areas of candidate activity and therefore their restricted newsworthiness to broader audiences or the fact that local candidacies are for some other reason not seen as interesting or legitimate topics.[3] In addition to the virtual absence of media coverage of individual candidates' campaigns, little or no attention is paid by the media to the activities of rank-and-file members of the Diet. What party leaders, cabinet members and the parties' policy spokesmen in the Diet do is reported. But the inner workings of Diet committees and the role of ordinary members of the Diet in processing of legislation or in other legislative functions go unnoticed.

Recent research in social cognition suggests that the differences between the communications processes concerning parties and candidates in Japan can be of profound importance to political attitude development and impact. According to some social psychological thinking, information processing complements people's attitudes by providing the rationales and internal argumentation that support and underwrite an affective orientation (Fiske and Taylor 1984). Both the regularity and amount of information that can be used to justify preferences—or in some instances change them—are important to these cognitive processes. Parties are a regular fare in the mass media and information about them is thematic. Repetition is believed to enhance the salience and vividness of particular information, which makes it easier to process. Information abundance permits more complete internal argumentation. Greater vividness and more thorough internal argumentation enhance the strength of particular attitudes and facilitate their recall from memory (Nisbet and Ross 1980). If individual Japanese find particular parties appealing, for whatever reason, the abundant amount of information available to them about parties permits more complete internal argumentation in support of the relevant attitudes. A deeper interpretive base in turn buttresses favorable attitudes and encourages their recall in situations where they are relevant to behavior.

In contrast with the abundance and regularity of messages about parties, information about candidates is only received on an intermittent basis by most people. Even though many candidates maintain permanent organizations that have regular meetings, these efforts involve only one out of every eight or so electors at best. For the remainder of the electorate, candidates are normally visible only at election time. Under these conditions of irregular and intermittent exposure, it is unlikely that many people could develop sufficient information to support robust attitudes even where they normally favor specific candidates at election time.

People thus learn about parties and candidates in different ways in Japan. Related differences in information processing suggest that partisan attitudes will be more robust than candidate orientations. In addition to the effects of the described differences in communications channels, the contexts in which people hear about parties and candidates vary in other ways that likely enhance the depth of attitudes toward parties relative to the richness of candidate impressions. Parties are active in most levels of elections in Japan. Independent candidacies are common only in rural and small city local elections. This means that people hear about parties each time there is an election, a process that makes it possible for partisanship and party imagery to deepen. Specific candidates run in only one kind of election at a time, with the consequence that their exposure is much more limited than in the case for parties, a factor that complements the effects of the overall intermittency of candidate exposure within particular kinds of elections.

Research Design and Data Sources

Strong arguments can be made for both partisan and constituency candidate models of the vote in Japan. The differential effects of attitudes toward partisan political objects and reactions to constituency candidates and campaigns is obviously a central question for Japanese voting behavior research. It is not a question that has been addressed satisfactorily by earlier research in Japan. Most scholarly studies of Japanese political behavior have focused on partisan variables, leaving to the newspapers and other polling efforts investigation of the effects of constituency campaigns; appreciation of the role of constituency factors in voting has been superficial as a result.[4]

The issue of partisan versus constituency components of the vote is addressed here in several stages. I begin by showing the simple distributions of voter contacts with campaigns and attitudes toward constituency and partisan political phenomena. The different clusters of examples of campaign exposure and attitudes toward candidates and parties are in turn grouped into a series of indexes that summarize their candidate or partisan direction. I test constituency campaign and partisan components models of voting, first separately, and then jointly. While undertaking these tests, I add various supplementary paths to the two models in order to portray as credibly as possible, within the scope of this inquiry, the relationships between different kinds of attitudes toward constituency candidates and parties and the effects of particular campaign experiences on attitudes. Finally, the implications of the findings are considered.

The data base for this analysis is the 1976 Japan General Election Study (JABISS survey).[5] The 1976 study contained extensive question batteries relating to constituency campaigns and their effects. These included information on local campaign contacts, political communications within informal social networks, group memberships and political preferences, and evaluations or images of candidates. Partisan elements of the election contest, party identification, party imagery, and issue opinions were also extensively covered.

Constituency Dimensions of Partisan Choice

Candidate Recognition

Voters in Japan's small, multimember House of Representatives districts usually have an opportunity to learn something about, or to have direct contact with, constituency candidates or be exposed to their efforts to mobilize the vote. This should be more true of constituencies in rural areas, provincial cities, and the older, small business portions of large cities than of the more impersonal middle class and working class residential neighborhoods in Japan's larger cities. But even where social structures are less congenial, candidates work hard to gain at least minimal recognition, using such techniques as widespread mailings of postcards between elections and extensive calling of candidates' names from loudspeakers on buses or automobiles that roam district streets and roads just before elections.

Name recognition itself should be very high in view of the extent to which the campaign is believed to center on the efforts of individual candidates. On the other hand, both the sheer number of candidates in many of Japan's multimember constituencies and the tendency of many candidates to concentrate their efforts in certain areas within specific constituencies dictate that people may not know the names of all district candidates. Our own strategy in the face of these complexities was to determine simply if people were able to remember the candidate for whom they voted. (The question was open-ended, so as to minimize inflation of responses through cuing.) Most of the voting electorate—actually four out of every five persons who turned out—identified valid candidates in their answers to this question. This finding indicates that candidates are at least minimally salient to most people in Japan.

Candidate Images

Evidence of other kinds indicated that the electorate was well aware of, or had extensive contacts with, the candidates and their constituency campaigns. Respondents to the 1976 survey were asked if a particular candidate from their district held opinions "close to their own views," was a "clean and fresh" candidate, would help their area, would help their occupation, or had "deep ties with the area" (Table 2). Nearly half of those persons who could name the candidate for whom they voted actually indicated at least one concrete image of a particular candidate. Seeing candidates as having "deep local ties" was the strongest theme, thus confirming the parochial focus of many voters. Moderate proportions of the voting public saw candidates as representing their occupation or having similar views. Surprisingly, very few people saw candidates as actually representing the local area, even though such a focus has been emphasized in descriptions of candidates' ties with their election bases (Curtis 1971; Richardson 1967). Since seeing a candidate as having deep local ties was much more common than feeling that a candidate would represent the local area, obligation and local loyalties may be stronger elements of the vote than expectations of performance.

Table 2. Candidate and Constituency Variables, 1976 Japanese General Election

Candidate and Constituency Variables	Percentage of Total[a]
Personal Requests for Voting Support	
Received request(s) from	
Family	5
Relatives	7
Coworkers	13
Neighbors	12
Friends	15
Received at least one request	36
Contact with Candidate	
Received postcard	44
Attended speech	18
Member of Koenkai	18
Had at least one contact	53
Candidate Image	
Felt candidate	
Had done something for local area	6
Had done something for respondent	1
Was "clean" and "fresh"	13
Had similar views to respondent	22
Had deep local ties	28
Would represent respondent's occupation	16
Had at least one favorable image	49

[a] Percentages are based on the total number of persons who could name the candidate for whom they had voted. *N* was 823 in all cases.

Source: 1976 JABISS survey (see n. 5).

Campaign Experiences

Electioneering by individual candidates is a major portion of parliamentary election activity in Japan. Just over 50% of the respondents who voted and knew which candidate they had supported reported having contacts with candidates' campaigns via attendence at speeches, receiving post cards, or joining a support organization (Table 2). Receiving post card greetings, reflecting an activity engaged in heavily by incumbent members of parliament at New Year's and at *chugen* in the midsummer, was the most common link between voter and candidate, outweighing speech attendence and support association membership by roughly three to one.

Interpersonal Requests

Japanese elections consist of both an open, formal campaign and an informal, less open effort in which candidates and their lieutenants personally contact voters and ask for their support, either directly or via social networks that include local elites and other persons (Flanagan 1971; Richardson 1979). Quite a few persons among those who had voted in the 1976 election had been contacted in this way.

Just over one third of the persons who remembered voting for a particular candidate had received a request to vote for one of the nominees from a family member, friend, relative, neighbor, or coworker.

All in all, nearly 60% of those who had voted in 1976 and knew the name of the candidate for whom they voted had at least one link with the candidate, such as a favorable image of the nominee or some kind of contact with the candidate, his organization, or the social networks mobilized in his favor. The frequency of activities and attitudes centering on particular candidates and reflecting the importance of constituency campaigns suggests a special prominence for candidates in Japan that fits popular and scholarly pictures of Japanese constituency politics.

Partisan Elements of Choice

Parties and partisan issues are also important to voters in Japan. Nevertheless, partisanship is itself fairly complicated and takes the form of a series of different but still partially overlapping phenomena. Some people in Japan are stable party identifiers, some are "image" partisans and some are habitual voters for the same party. Many voters manifest combinations of the three different partisan orientations.

Party Identification

Party identification is well known as the core concept of U.S. and comparative political psychology and is normally seen as an effective party tie. In the JABISS survey, respondents were asked if they "supported" one of the six significant parties running candidates in the 1976 election. This question was as close a replica of the traditional U.S. party identification item as is possible and was asked in the same form on both panel waves. Four out of five of the JABISS respondents who had voted indicated a party identification on the first wave.[6] This figure increased substantially in the second panel wave interviews. However, only 59% of the persons who voted were actually stable identifiers over the two panel waves. The remaining 41% wavered between an identification and an apolitical stance of *no party* or *don't know*—the dominant alternative pattern—actually switched parties between interviews, or were consistently apolitical (see also Flanagan and McDonald 1979; Richardson 1986). This figure for stable self-conscious partisanship seemed very low, given that the period between the two panel waves was only one month. As various studies of party identification in Japan (Flanagan and McDonald 1979; Miyake 1970, 1985, 1986a; Miyake, Kinoshita, and Aiba 1967; Richardson 1975) have noted, lower levels of stability are normal for Japanese party loyalties relative to those in other countries, regardless of whether the intervening time intervals are brief or long. This finding has always led to a discounting of the importance of affective party identifications for the Japanese voter.

Habitual Voting

My earlier research (Richardson 1986) showed that consistent voting for the same party in the past plays an important role in electoral choice in later elections in Japan. For a significant portion of the electorate—at least one voter out of five—habitual voting, measured by reports of voting for the same party in the last three national and local elections prior to the 1976 contest, is the main evidence of a stable party tie. For these people, a voting habit takes the place of a stable party identification, since their answers to the party identification item indicated an apolitical stance, wavering between independence and a partisan commitment, or shifting preferences between the first and second wave. For most other persons habitual voting combines with stable party identifications as evidence of continuous partisan attachments reinforced by conforming behavior. When both groups of partisans—stable identifiers in the two waves and persons voting consistently in past elections—are accounted for, 85% of the Japanese voting public in 1976 indicated some kind of stable party attachment (Table 3).

Party Images

Party images, a third area of the electorate's response to parties, is fairly widespread in the Japanese case (see also Kohei 1979; Miyake 1986b). Party images are

Table 3. Partisan Factors, 1976 Japanese General Election

Partisan Factors	Percentage of Total[a]
Party Identification and Partisan Vote Consistency	
Identified with a party in first wave	82
Identified with same party both waves	59
Voted consistently in recent elections[b]	82
Voted consistently and/or stable identifier	85
Party Image	
Felt party was	
Best in candidates	64
Best in leaders	54
Best in policy	52
Best in occupation representation	61
Best in local area representation	52
Best to end corruption	47
Best to stop inflation	44
Best to govern	59
Had at least one favorable image	85
Closest Party	
Felt some party closest to own position on	
Emperor's role in government	36
U.S.-Japanese security treaty	43
China treaty	40
Korean relations	29
Northern territories	43
Defense forces	43
Business tyranny	46
Social welfare	51
Right to strike	44
Growth vs. inflation	35
Money politics	47
Political contributions	42
Had at least one nomination of a "closest party"	73

[a] Percentages are based on the total number of persons who could name the candidate for whom they had voted. $N = 823$.

[b] Voting consistency in past three elections includes persons who voted in three out of three elections for same party, persons who voted in two out of three elections for same party while voting three times or those who voted only twice but for same party.

understood to be people's broad, general cognitive pictures of the individual political parties and have been seen by some scholars as major, independent forces in voting behavior in addition to party identifications (Baker, Dalton, and Hildebrandt 1981; Butler and Stokes 1969; Matthews and Prothro 1966). In the JABISS survey images of parties were sought through closed questions regarding which parties were seen as having the best candidates, leaders, or policies; which parties were most capable of representing local or occupational interests; and which parties could best stop inflation, end corruption, or rule Japan. (Each of the eight dimensions of party images covered by the JABISS survey reflected themes in people's party images identified in earlier Japanese studies).

Eighty-five percent of the electorate had at least one favorable cognitive image of a political party, a figure substantially higher than that for stable party identifications (Table 3). The most commonly held image was that of a party "having the best candidates." Sixty-four percent of the voting electorate possessed a favorable image of one of the parties on that dimension. But nominations of parties "best suited to govern" and "best representing my occupation" were close runners-up.[7] Images were also well represented in the electorate's aggregate responses to parties. Forty percent of the 1976 voting electorate were stable party identifiers, had favorable images of their preferred party, and had voted for that party regularly. An additional 19% combined favorable images with either stable identifications or habitual voting and 5% had only favorable images of a preferred party.

Partisan Issue Opinions

Partisan issue domains are recognized by many voters in Japan, although partisan issues were less salient to the electorate in 1976 than the more general aspects of the parties captured in party image questions. Voters were asked which party they felt was closest to their own opinions on twelve domestic and foreign policy questions, including the emperor's role in government, security and foreign policy, and domestic policy problems such as the economy, social welfare, and political corruption (see Table 3).[8] Seventy-three percent of the voting public felt that a particular party was closest to their position on at least one of the twelve issues. Half of the voting electorate could nominate a party as closest to their own positions on the single most salient issue, which was the adequacy of social welfare programs. Still, *don't know* answers were systematically higher in the case of replies to the more specifically focused issue proximity questions than on more broadly framed party image items, and fewer voters felt parties were close to their own ideas on multiple issues than had multiple images of the parties. However, once the deficiencies in party identifications and a little flabbiness in feelings of issue proximity are noted, the political parties have obviously cumulatively penetrated the electorate on a substantial variety of dimensions in Japan.

Comparing Party and Candidate Images

One of the significant patterns uncovered by this research was the presence of very large differences in the raw frequencies of multiple images of candidate and party respectively (Table 4). Seventy-eight percent of the Japanese voting electorate in 1976 reported two or more favorable images of some party, and another 7% possessed at least one image. Only 15% of those who voted lacked any favorable image of a party. In contrast, 51% of those who voted had no favorable image of a candidate, and an additional

25% had just one image. Only 24% had two or more favorable candidate images, in contrast with the 78% level for party images! Cognitive pictures of parties are obviously much more richly developed than those of candidates, reflecting in large part the differences in communications processes discussed above. People know and like a lot more about parties than they do candidates, in part because they see, read, and hear so much more about them on a regular basis. Parties in Japan aren't as remote and disheartening as has been thought in the past, a fact likely relevant to how people vote.

Table 4. Multiple Images of Candidate and Party

Images	Percentage of Total[a]
Candidate	
Two or more	24
One	25
None	51
Party	
Two or more	78
One	7
None	15

[a]Figures for candidate and party images are simple response frequencies for all persons who voted for a specific candidate. $N = 823$.

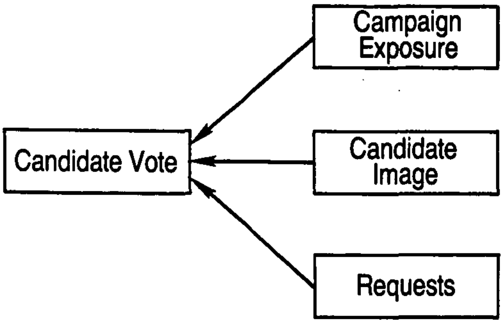
Are There Candidate Voting Traditions?

Given the importance of candidates and their campaigns in Japan, is it possible that there are candidate voting traditions that resemble habitual party voting, or candidate loyalties akin to party identification, even though candidate images are more shallow than mental pictures of parties? Can a more latent form of candidate orientation than images be uncovered? Apparently the answer is *no*. Information from questions about people's self-perceptions of their past voting behavior suggest that a candidate voting habit comparable to partisan voting habits and identifications was simply not present. Respondents were asked before the election if they "normally voted for the same candidate." Surprisingly, in view of the election mobilization literatures' emphasis on candidate campaigning and the high incumbency rates in relevant Diet elections, only 24% of the persons who subsequently voted said they had voted regularly for the same person in the past. In contrast, after the election, when a similar question was asked about partisan voting, three-quarters of the respondents who had voted in 1976 said they had "voted for the same party over time." Although, the two figures are not totally comparable because of differences in question format and timing, the differences between the frequencies of persons who reported having voted consistently for a candidate and those who said they always voted for the same party are so great that far more voters appear to be bound to parties than to candidates.

Models of the Vote

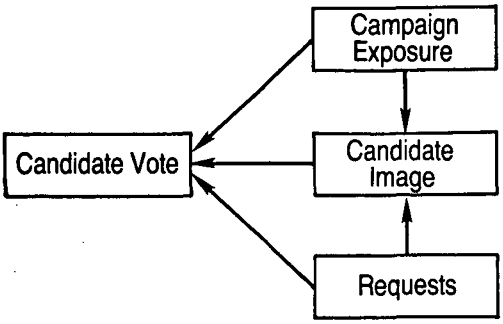
A logical first step toward evaluating the effects of partisan and constituency components of Japanese voting behavior is to see how responses to constituency candidates and their campaigns sort out as determinants of the vote. A model of the effects of the campaign and images of the candidate is proposed in Figure 1. Three aspects of the voting public's exposure to local campaigns have been identified: candidate images, direct contacts with candidates' campaigns, and receipt of requests for a supportive vote. Each of these dimensions of voter response to constituency candidates and campaigns are postulated as directly affecting voting choices. In addition to their direct effects on voting, campaign activities and requests themselves probably help people

Figure 1. A Simple Constituency Model



form candidate images in certain cases. The effects of these experiences are especially important since so little media attention is paid to constituency candidates. The causal diagram shown in Figure 2 contains additional paths so that the indirect effects of campaign experiences and requests on images of candidates can be taken into account. Looking at things this way should help round out our understanding of how people learn about constituency candidates. (Obviously, incumbents and candidates who ran in earlier elections also may benefit from the effects of prior campaigns and efforts to maintain stable followings between elections. Some of these processes are tapped by our questions about post cards and membership in support associations, but some important interelection informal communications processes lie beyond the scope of our survey).

Figure 2. An Elaborated Constituency Model



A simple causal model of the partisan forces influencing the 1976 parliamentary vote is shown in Figure 3. Stable party identifications, consistent past election choices, favorable party images and feelings of issue proximity—the four ways in which Japanese respond to political parties—are each hypothesized to affect the vote. In addition to their direct effects on the vote, it is likely that the various attitudes represented as independent variables are casually linked to each other. But deciding on the precise causal role of some variables, especially the personal voting habits represented in consistent past voting, was not easy. The behaviors summarized in the voting habit index pretional impact on the latter. But questions about contemporary party identifications presumably tapped *durable* partisan attachments that earlier guided the voting choices. Much of the same logic applies to party images. For these reasons, party images and identifications are seen as causing past voting behavior in the expanded model of partisan components of the vote (Figure 4), even though in certain individual cases causality over time was a more complicated matter.

The models developed thus far have dealt with attitudes toward parties and responses to local candidates and their campaigns as if these were completely separate entities. Nothing is more unlikely. Party loyalties and favorable appraisals of local candidates are undoubtedly intertwined in people's thinking in many ways. They work together to influence how people vote, as is outlined by the third main variant of our causal models (Figure 5). In addition, attitudes toward local candidates undoubtedly affect feelings toward political parties and vice versa. Japanese parties historically began as coalitions of local as well as national

Figure 3. A Simple Partisan Model

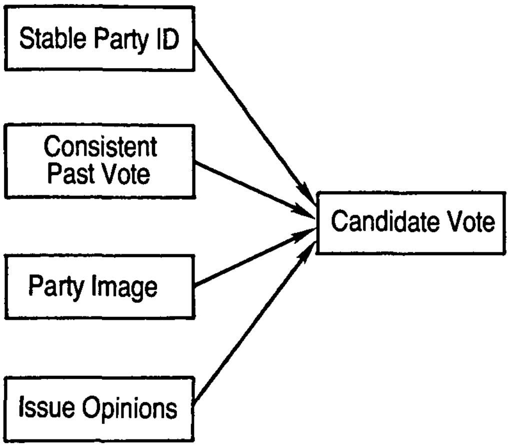
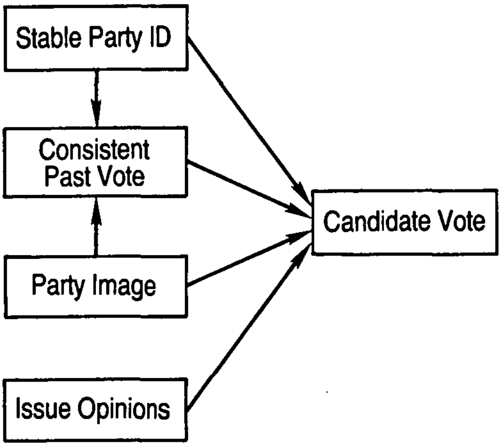

Stable Party ID
Consistent Past Vote
Party Image
Issue Opinions
Candidate Vote

Figure 4. An Elaborated Partisan Model


Stable Party ID
Consistent Past Vote
Party Image
Issue Opinions
Candidate Vote

politicians and interests, and remain intimately linked as informal organizations to their constituency bases today. It is interesting in this context to see the extent to which constituency and candidate considerations enter people's normal frame of reference when thinking about parties. Analysis of answers to different items in our closed question battery on party images showed that feelings that specific parties "have good candidates"—a constituency candidate referent in House of Representatives elections and House of Councilors local district contests—was highly correlated with the remaining dimensions of images, which had an exclusively partisan focus. In other words, the two dimensions are tightly integrated. There was no significant segment of the public that thought of the parties exclu-

Figure 5. A Comprehensive Partisan-Constituency Candidate Model

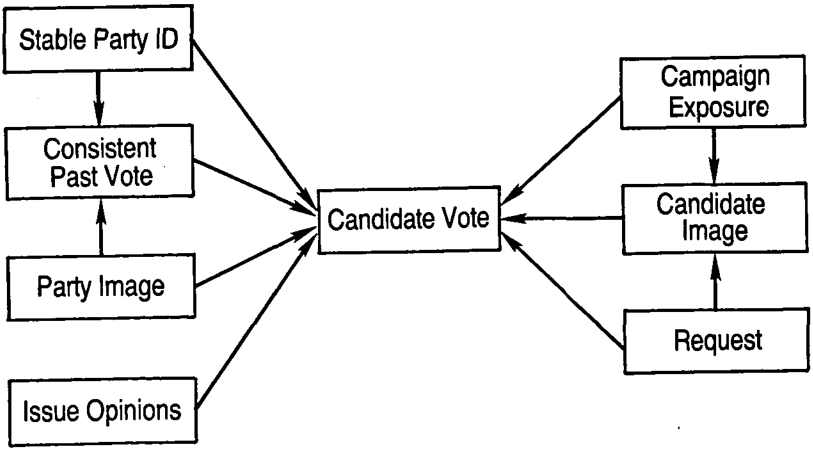
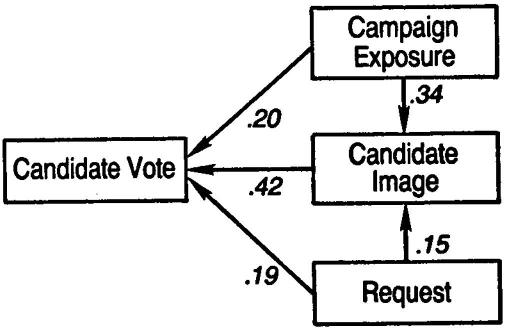

Stable Party ID
Consistent Past Vote
Party Image
Issue Opinions
Candidate Vote
Campaign Exposure
Candidate Image
Request

Figure 6. Constituency Components of Voting Choice



Summary Constituency Components	Paths Direct	 Indirect	Total
Candidate images	.42	—	.42
Campaign exposure	.20	.14	.34
Interpersonal requests	.19	.06	.25

Note: All numbers are standardized beta coefficients. $N = 822$; $R^2 = .37$.

sively in constituency candidate terms. We are led to believe that cognitive images of the parties in Japan are mainly focused on the parties' general characteristics and that constituency candidates enter into the picture mainly as integrated components of a broadly focused imagery.

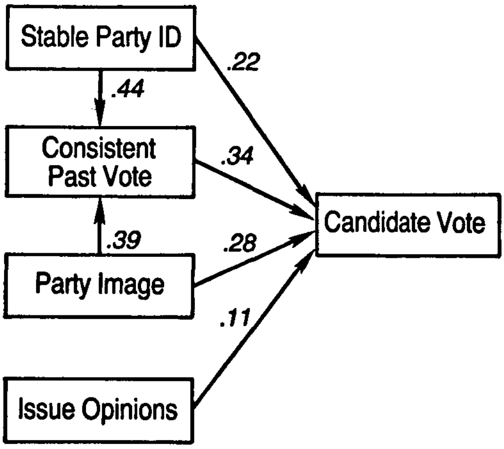
Various additional analyses were undertaken to see what direct effects constituency candidate images might have on attitudes toward parties and how partisan attitudes might affect feelings toward candidates. Clearly, persons who favor the candidates of a particular party supported the same party in most instances. But only a cross-time analysis of these relationships could really determine directionality of the effects on partisan attitudes of feelings toward candidates and vice versa. For this reason, there are no arrows for the relationships between candidates and partisan attitudes in the causal model proposed here.[9]

Testing Models of the Vote in Japan

In Figure 6, the results of a multiple regression of the vote for candidate on the three core components of voter reactions to local constituency campaigns are shown. Two things stand out in this simple analysis of constituency and candidate influences on the vote. First, the three aspects of voter experience with constituency candidates and their campaigns do a pretty good job in explaining candidate choice. Altogether, 37% of the variance in the candidate vote is accounted for by the three core variables. Constituency variables are important in Japan.

The second notable feature of the regression findings in Figure 6 concerns the effects of the different aspects of voters' campaign experiences and attitudes. Candidate imagery is more important than either campaign experiences or informal requests for one's vote. Never-

Figure 7. Partisan Components of the Vote Decision



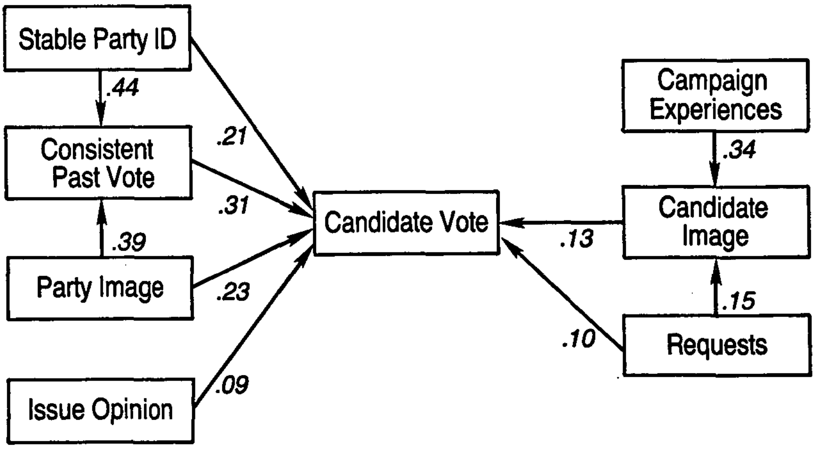
Summary Partisan Components	Paths Direct	Paths Indirect	Total
Party images	.28	.13	.41
Stable party identification	.22	.15	.37
Consistent past voting	.34	—	.34
Issue opinions	.11	—	.11

Note: All numbers are standardized beta coefficients. $N = 822$; $R^2 = .67$.

theless, the direct consequences of efforts to mobilize the electorate via requests and campaign contacts were still significant. Moreover, having received a request and being exposed to the campaign—receiving a post card, attending a speech, or joining a support association—had substantial indirect effects on candidate images, as predicted in the elaborated model outlined in Figure 2, and their total effects on voting choices were enhanced as a result.

The analysis in Figure 6 shows that stronger indirect relationships exist between campaign experiences and candidate images than between requests and candidate images. Prior attraction to particular candidates certainly motivates some people to join support organizations or attend speeches, which means that the attitude linkages have already been made in the past, even though we don't know their direction. The time spans implicit in campaign contacts are also longer than those involved in requests, and the specific forms of exposure are themselves repeatable. Campaign experiences consequently imply greater possibilities for reinforcement than requests, since the requests we asked about were only those occurring during the 1976 campaign. Finally, as I have shown elsewhere (Richardson 1966), requests in urban areas are often simply a recommendation of a particular candidate accompanied by no commentary, while in rural areas requests are usually "rationalized" by favorable comments about the candidate. This means that requests are lacking in sub-

Figure 8. Candidate, Constituency, and Party in Voting Choice



Summary	Paths Direct	 Indirect	Total
Party images	.23	.12	.35
Stable party identification	.21	.13	.34
Consistent past voting	.31	—	.31
Candidate images	.13	—	.13
Interpersonal requests	.10	.02	.12
Issue opinions	.09	—	.09
Campaign experiences	—	.04	.04

Note: All numbers are standardized beta coefficients. $N = 823$; $R^2 = .70$.

stance for many people, which could explain why they have such a restricted effect on candidate images.

Estimates of the partisan components model developed above indicated that the strongest direct influence on voting was a consistent voting record in the past (Figure 7). Party images and stable party identifications were also significant to the vote, contributing nearly as much to voting choices directly as did consistent past voting. When the indirect effects of party imagery are added to their direct impact, party images are the strongest single determinant of the vote. Stable party identifications are second in importance. This analysis demonstrates that other party variables, namely party images and consistent voting in the past (which is best seen as evidence of a voting habit), join party identifications as major factors in the 1976 vote choice in Japan. Clearly, each separate dimension of people's responses to parties has an important contribution, contrary to the expectations of at least traditional political psychology that party identifications would be the dominant influence. Meanwhile, feelings of issue proximity toward the parties, although far from insignificant, had a much weaker impact than the three alternative indications of partisan preferences

(Figure 7). Importantly, the partisan components model explained 67% of the variance in the 1976 candidate vote, or nearly twice the contribution attributable to candidate images and constituency campaigns.

The final test of the contribution of voters' reactions to consistency candidate and partisan aspects of elections, consisting of an examination of their combined effects on the vote, is summarized in Figure 8. Despite the great emphasis in the extant Japanese election literature on the importance of constituency campaigns, variables representing partisan attitudes and habits—party identification, party images, and voting habits in past elections—were the principal factors influencing the 1976 Japanese vote.

In addition to the overall domination of voting choices by party attitudes and past voting experiences, the contribution of candidate images and campaigns was somewhat lower in the combined models tests than in the case of the constituency model reported above. Most people favored candidates from their own preferred party, or were exposed mainly to the campaigns of candidates from their preferred party (see also Richardson 1979). Candidate choices were thus made under the "umbrella" of a partisan identification or some kind of partisan tie, a finding that severely qualifies the longstanding evidence from simpler analyses (Table 1) that candidate is a major, independent factor in Japanese electoral behavior nearly equal in importance to party. (The path representing the contribution of campaign experiences to voting is dropped in Figure 8, as the relevant relationship failed to meet tests of significance.)

Interpretation and Implications

This analysis has shown that a partisan components model of the vote explains over twice as much variance in Japanese parliamentary voting as a constituency candidate model. The omnipresent local constituency campaigns in Japan have relatively subdued effects on voting relative to partisan components of choice. Much of the attention to voting in Japan on the part of U.S. scholars (e.g., Flanagan and McDonald 1979; Richardson 1975) has been distracted by the weak party identification phenomenon to the point of ignoring the importance of parties to Japanese voting. The weakness of party identification made sense in light of folklore about the importance of local candidate organizations in Japan (Richardson 1975). But this led us to ignore two extremely important aspects of the Japanese electorate's reactions to parties: party images and consistent voting habits.

Party images—broad cognitive pictures that people develop of political parties—have been suggested as an important form of electorate response to parties in earlier writings on the United States (Matthews and Prothro 1966; Trilling 1976; Wattenberg 1981) and Europe (Baker, Dalton, and Hildebrandt 1981; Butler and Stokes 1969; Wattenberg, 1982). But the status of party images has never been completely clear. To some scholars they are mainly correlates of party identifications; to others they are seen as independently important. In Japan both versions of party images are found. Many identifiers have complementary images of their favored parties. In addition, quite a few people—actually 13% of the voting public—maintain favorable images of parties without being firm identifiers. It is this group within the electorate that gives images a special importance in Japan. The presence of both groups of image partisans enhances the role of cognitive partisanship in a culture where affective partisanship is relatively weak and unstable.

I have already reported the presence of a consistent voting habit in Japan—a second form of electoral response to parties different from party identification (Richardson 1986). My analysis here shows more clearly how independently important the effects of this habit actually are relative to those of stable party identifications. This analysis has shown that Japanese voters react to political parties in three partially distinct and partially overlapping ways. Once the variation in the electorate's reactions to parties in Japan is recognized, the importance of parties in Japanese electoral behavior becomes clear.

Partisan components of parliamentary voting have been shown far to outweigh the effects of constituency campaigns and candidates in Japan. Even in a country where constituency factors should be unusually important, attitudes toward parties and party-oriented voting habits were the dominant factors in national election voting. The findings of this research, including evidence of dramatically greater cognitive depth for party than for candidate images, severely qualify much earlier interpretation of Japanese voting. The research also suggests that the communications channels through which information about parties and candidates is acquired merit further study, as do related processes of social change and media diffusion. Parliamentary voting in Japan appears to be substantially different from congressional voting in the United States, the main available comparative referent at present. Constituency candidate evaluations are very important in the U.S. case, whereas parliamentary voting in Japan is dominated more by attitudes toward national parties. Quite a bit of these differences between countries may reflect significant cross-national variations in political communications.

Methodological Appendix

Index Construction. Summary indexes were created for candidate images, requests, constituency campaign exposure, party images, issue opinions, habitual voting, and stable party identification. Each candidate index summarizes the specific candidate direction of a plurality or more of each individual's replies in the relevant content areas. Both the candidate indexes and the candidate vote choice variables were coded arbitrarily as "first LDP candidate," "second LDP candidate," and so on for each election district. (The Communist party, Clean Government party, New Liberals, and Democratic Socialist party did not run plural candidates and were represented by only one candidate code.) Using this procedure and the resulting two-digit code, the responses favoring a particular candidate from among the individual election district alternatives could be analyzed among voters in specific constituencies throughout Japan without reference to district.

Party images, habitual voting, and partisan issue opinions were represented by summary indexes that were coded simply as summary, plurality preferences for one of the six main parties active in the 1976 election—the Liberal Democrats, New Liberals, Democratic Socialists, Clean Government party, Japan Socialists, and Japan Communists. All other responses were treated as missing data so that single-dimensional scales of partisanship suitable for analysis of behavior in multiparty systems could be constructed. Since vote for specific candidates was the dependent variable throughout the analysis in this article, the party codes were arbitrarily recoded to the midpoint of the range of the multiple candidate codes; for example, since there were four codes for Liberal Democratic candidates (11, 12, 13, 14), preferences for the Liberal Democratic on indicators for party variables were coded to 12.5. Party identification was coded simply as the party preferred in both waves, with all other combinations being treated as missing data.

Missing Data. Missing data constitute a major problem in Japanese political behavior research, sometimes amounting to as much as one-third or one-half of responses. Exclusion of missing data from the analysis in these circumstances results in a distorted view of the Japanese public as a whole, and also presents some statistical problems. Since this analysis addressed aspects of the behavior of all Japanese voters, missing data were coded to the means for each variable and treated as valid codes. This practice tended to depress inter-item correlations and the standardized betas used in the path analyses. For comparison and validation, regressions were run in which missing data were deleted using both pairwise and listwise procedures. Tests employing the candidate and constituency variables using this alternative approach were unacceptable because of extremely small numbers. Those using the party variables produced distorted findings due to heightened multicollinearity (which has not been a problem in earlier runs using the mean for missing data). Importantly, using alternative approaches to missing data had no effect on the relative weights of party versus constituency variables, so the general findings of this analysis were unaffected.

Explained Variance. A problem of multiple regression analysis is a tendency for strong variables to dominate other variables to a degree out of proportion to their actual contribution. In order to be sure that our results were valid, stepwise regressions were run in different ways so as to compensate for the effects of the stronger partisan components variables, and the sequential explanations of variance in the different combinations were compared. In one run, the candidate and campaign variables were introduced first; subsequent addition of the partisan components variables added 33% to explained variance. When the partisan components were entered first, inclusion of candidate and campaign variables added only a further 3% in explained variance. Clearly, the partisan variables provide the most definitive explanation of parliamentary voting in Japan.

Measurement Validity. The answers to the closed questions employed to identify party and candidate images in the JABISS survey are clearly reasonably close to the decisions involved in the vote itself, as is witnessed by the high proportions of variance in the vote attributable to the two image variables. To some students of voting, the relationship between images and the vote is believed to be tautological or nearly so (Heath, Jowell, and Curtice 1985), and, according to their views, alternative variables should be employed. Several answers can be made to this potential criticism. The party images used in this analysis are linked to party identifications measured at two points in time, to recall of consistent voting in the past, or to both of these presumably stable propensities in over 90% of their occurrences. (I estimated recall of consistent past voting itself to be valid in typically over 90% of its occurrences [Richardson 1986]). Because of these linkages of party images to other long-term variables, people's party images can be assumed generally to have intermediate or long-term status. For this reason, our use of party images is not broadly vulnerable to the criticism that its relationship to the vote is tautological. In addition, as was noted earlier, multiple party images outnumbered multiple candidate images at a ratio of three to one. A difference of this magnitude should itself indicate a substantial degree of validity for both measures. Finally, psychological variables such as party and candidate images are necessary components of Japanese political behavior analysis. They cannot be replaced by social class or other "sociological" variables, which are sometimes claimed to be causally more distant from the vote, given the relative insignificance in Japanese political life of societal cleavages (Flanagan and Richardson 1977; Watanuki 1988).

Notes

The 1976 Japanese General Election Survey upon which this report is based was supported by grants from the National Science Foundation and the Hoso Bunka Kikin. Special appreciation is due James Ludwig of the Polimetrics Laboratory of Ohio State University for assistance in data management and Aiji Tanaka and Gregory Tilton for help with data analysis.

1. Recent research on candidate visibility and related issues in France in Converse and Pierce (1986) and the Japanese Election Study (JES) series of projects (Watanuki 1986a, 1986b) are exceptions.

2. One hundred councilors are elected in a single nationwide constituency, fifty each in elections at three-year intervals. Proportional representation based on the d'Hondt rule replaced the single nontransferable vote there in 1983. The remaining councilors are elected in local districts in Japan's 47 prefectures.

3. Japanese election constituencies are themselves small. Candidate's campaigns are also often geographically focused within each election district. A given prefecture has typically between three and four constituencies and within each constituency several individual candidate support "territories" may exist.

Although the national dailies and regional papers do carry long series of articles on local campaigns, these rarely go beyond brief references to each candidate and his organizational activities.

4. An excellent and very thorough study of both partisan and constituency components of voting in Japan based on a local sample is Miyake, Kinoshita, and Aiba 1967, which deals with behavior in several different levels of elections in the city of Uji in Kyoto prefecture. Two recent landmark studies also report important findings on the role of partisan or candidate components of the vote in great depth (Watanuki et al. 1986a, 1986b) but do not directly broach the combination and interaction of national and local forces as completely as is done here. Other studies of the contest between candidate and partisan influences have been conducted by two semiprivate groups—the Komei Senkyo Remmei and its successor organ, the Akarui Senkyo Suishan Kyokai—and affiliated local groups. While providing valuable trend information, these studies have dealt with constituency campaigns and related phenomena by means of relatively superficial inventories of self-reported motivations and campaign experiences, such as those reported in Table 1.

5. The acronym is based on the first initials of the principal investigators, Joji Watanuki, myself, Ichiro Miyake, Scott Flanagan, and Shinsaku Kohei. The JABISS survey utilized a nationwide sample that was stratified with regard to urban-rural residence. Selection of individual respondents was made randomly. Interviews were conducted in two panel waves; the analysis reported here was based on the panel portion of the respondents, whose total number was 1,235.

6. The frequency for first-wave party identifications is calculated using the segment of the total sample who reported voting for a specific candidate as the base, as are all other statistics reported in this article. Since people who knew the name of the candidate they had supported were more politically aware than persons who voted but could not identify the candidate of their choice, marginal frequencies for party identification and other variables are slightly higher than those reported elsewhere (e.g., Flanagan and McDonald 1979) for the total voting public or the entire JABISS sample.

7. Since party images were solicited only in the postelection wave, their stability is unknown. However, images were linked closely with one or the other of the two stable party responses; a summary index of respondent dispositions on all eight image dimensions showed correlations of .687 with habitual voting and .657 with stable party identification.

8. The form of the partisan issue proximity questions employed in the JABISS survey could have invited projection of partisan attachments onto issue opinions. But there were actually substantially lower frequencies of feelings that there were parties close to one's position than of willingness to place oneself on position issue scales, which leads us to believe that projection was not as much a problem as might be anticipated. In addition, there are some justifications for using a proximity-type question in the Japanese case. Alternative approaches to measurement of issue opinions and issue-party linkages, such as using issue position questions, are less workable in Japan than in simpler party systems. Issue differences between Japan's Middle and Left parties are very complicated, due to different alignments on different issues and the absence of measurable differences between some parties on some issues; and these complexities favor use of a simple measure of partisan issue linkages, like the proximity question.

9. Estimation of a nonrecursive model of these relationships was not undertaken due to the difficulty of identifying a sufficient number of exogenous variables in the context of Japan's complicated and asymmetrical "social matrix" patterns of partisan support (Watanuki 1988).

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CONTRASTING RATIONAL AND PSYCHOLOGICAL ANALYSES OF POLITICAL CHOICE

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We contrast the rational theory of choice in the form of expected utility theory with descriptive psychological analysis in the form of prospect theory, using problems involving the choice between political candidates and public referendum issues. The results showed that the assumptions underlying the classical theory of risky choice are systematically violated in the manner predicted by prospect theory. In particular, our respondents exhibited risk aversion in the domain of gains, risk seeking in the domain of losses, and a greater sensitivity to losses than to gains. This is consistent with the advantage of the incumbent under normal conditions and the potential advantage of the challenger in bad times. The results further show how a shift in the reference point could lead to reversals of preferences in the evaluation of political and economic options, contrary to the assumption of invariance. Finally, we contrast the normative and descriptive analyses of uncertainty in choice and address the rationality of voting.

The assumption of individual rationality plays a central role in the social sciences, especially in economics and political science. Indeed, it is commonly assumed that most if not all economic and political agents obey the maxims of consistency and coherence leading to the maximization of utility. This notion has been captured by several models that constitute the rational theory of choice including the expected utility model for decision making under risk, the riskless theory of choice among commodity bundles, and the Bayesian theory for the updating of belief. These models employ different assumptions about the nature of the options and the information available to the decision maker, but they all adopt the principles of coherence and invariance that underlie the prevailing notion of rationality.

The rational theory of choice has been used to prescribe action as well as to describe the behavior of consumers, entrepreneurs, voters, and politicians. The use of the rational theory as a descriptive model has been defended on the grounds that people are generally effective in pursuing their goals, that the axioms underlying the theory are intuitively compelling, and that evolution and competition favor rational individuals over less rational ones. The objections to the rationality assumption were primarily psychological. The human animal, it has been argued, is often controlled by emotions and desires that do not fit the model of calculating rationality. More recent objections to the maximization doctrine have been cognitive rather than motivational. Following the seminal work of Herbert Simon (1955, 1978) and the emergence of

AMERICAN POLITICAL SCIENCE REVIEW
VOL. 82 NO. 3 SEPTEMBER 1988

cognitive psychology, it has become evident that human rationality is bounded by limitations on memory and computational capabilities. Furthermore, the experimental analysis of inference and choice has revealed that the cognitive machinery underlying human judgment and decision making is often inconsistent with the maxims of rationality. These observations have led to the development of a descriptive analysis of judgment and choice that departs from the rational theory in many significant respects (see, e.g., Abelson and Levi 1985; Dawes 1988; Kahneman, Slovic, and Tversky 1982; Tversky and Kahneman 1986).

We contrast the rational theory of choice with a descriptive psychological analysis, using a series of questions involving political candidates and public referenda. These problems are used to illustrate the differences between rational and descriptive theories of choice and to test their predictions. Some of the questions probed our respondents' views about familiar political issues, such as the Equal Rights Amendment and the prevalence of crime in black neighborhoods compared to white neighborhoods. In other cases involving the test of general hypotheses, such as risk aversion, we introduced hypothetical problems in order to achieve experimental control and eliminate the influence of irrelevant factors. The use of hypothetical problems raises obvious questions regarding the generality and the applicability of the finding. Nevertheless, we believe that the use of carefully worded questions can address key issues regarding people's values and beliefs so long as respondents take the questions seriously and have no particular reason to disguise or misrepresent their true preferences. Under these conditions hypothetical questions can be used to compare alternative theories of political choice that cannot be readily tested using available survey and voting data. Our results, of course, do not provide definitive conclusions about political decision making, but they may shed light on the formation of political judgment and stimulate new hypotheses that can be tested in national election surveys in the years to come.

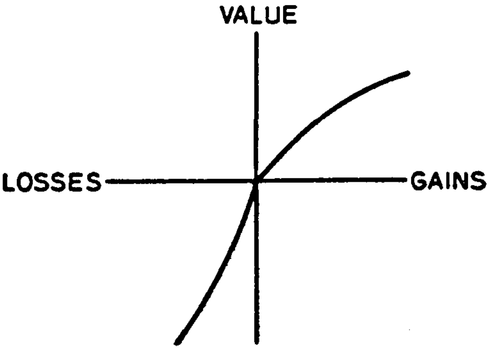
We focus on expected utility theory, which is the major normative theory of decision making under risk (von Neumann and Morgenstern 1947; Raiffa 1968; Savage 1954). This model is contrasted with prospect theory, a descriptive analysis developed by Kahneman and Tversky (1979, 1984). The first section deals with the role of the reference point and its impact on the choice between political candidates. In the second section we test the assumption of invariance and contrast it with a psychophysical analysis of numerical scales. The third section deals with the perception and the weighting of chance events, and the role of uncertainty in choice. The fourth section addresses the classical issue of the rationality of voting. It contrasts, again, a rational analysis based on the probability of casting a decisive vote with a less rational analysis that incorporates an element of self-deception. The implications of the present analysis are discussed in the fifth and final section.

Reference Effects, Risk Attitudes, and Loss Aversion

The standard utility function, derived from the expected utility model, has two essential characteristics. First, it is defined on wealth, or final asset position. Thus, a person with wealth *W* accepts an even chance to win $1,000 or lose $500 if the difference between the utility of W + $1,000 and the utility of *W* (the upside) exceeds the difference between the utility of *W* and the utility of W − $500 (the downside). Second, the utility function is concave; that is, the subjective value of an additional dollar diminishes with the total amount of money one has. The first assumption (asset integration) is necessitated by basic considerations of coherence. The second assumption (concavity) was introduced by Bernoulli (1954) to accommodate the common observations of risk aversion, and it has played an essential role in economics. A person is risk-averse if he or she prefers a sure outcome over a risky prospect that has an equal or greater expected value. For example, most people prefer $100 for sure over an even chance to win $200 or nothing. Risk aversion is implied by the concavity of the utility scale because the utility of $2x$ is less than twice the utility of x.

Although risk aversion is quite common, particularly for prospects with positive outcomes, risk seeking is also prevalent, particularly for prospects with negative outcomes. For example, most people find a sure loss of $100 more aversive than an even chance to lose $200 or nothing. To explain the combination of risk aversion and risk seeking, prospect theory replaces the traditional concave utility function for wealth by an S-shaped function for changes of wealth. In this theory, therefore, the carriers of values are positive or negative changes (i.e., gains and losses) defined relative to a neutral reference point. Furthermore, the value function is assumed to be concave above the reference point and convex below it, giving rise to risk aversion in the domain of gains and risk seeking in the domain of losses. As in the classical theory, it is assumed that the difference between $100 and $200 is subjectively larger than the (numerically equivalent) difference between $1,100 and $1,200. Unlike the classical theory, however, it is assumed that the difference between a loss of $100 and a loss of $200 is subjectively larger than the numerically equivalent difference between a loss of $1,100 and a loss of $1,200. Thus, the value function of prospect theory is steepest at the origin and it gets shallower as one moves away from the reference point in either direction. An important property of the value function—called loss aversion—is that the downside is considerably steeper than the upside; that is, losses loom larger than the corresponding gains. A typical value function with these characteristics is given in Figure 1.

Figure 1. A Hypothetical Value Function



Attitudes towards Risk

Expected utility theory and prospect theory yield different predictions. The classical theory predicts risk aversion independent of the reference point, whereas prospect theory predicts risk aversion in the domain of gains and risk seeking in the domain of losses (except for small probabilities). Furthermore, prospect theory implies that shifts in the reference point induced by the framing of the problem will have predictable effects on people's risk preferences. These phenomena are illustrated in the following four problems, each involving a choice between alternative political prospects.

The respondents to these and other problems reported in this article were undergraduates at Stanford University or at the University of California at Berkeley. The problems were presented in a questionnaire in a classroom setting. Each problem involved a simple choice between two candidates or positions on a public referendum. The respondents were asked to imagine actually facing the choice described, and they were assured that the responses were anonymous and that there were no correct or incorrect answers. The number of respondents in this and all subsequent problems is denoted by *N*, and the percentage who chose each outcome is given in parentheses.

Problem 1 (*N* = 89)

Suppose there is a continent consisting of five nations, Alpha, Beta, Gamma, Delta, and Epsilon. The nations all have very similar systems of government and economics, are members of a continental common market, and are therefore expected to produce very similar standards of living and rates of inflation. Imagine you are a citizen of Alpha, which is about to hold its presidential election. The two presidential candidates, Brown and Green, differ from each other primarily in the policies they are known to favor and are sure to implement. These policies were studied by Alpha's two leading economists, who are of equal expertise and are impartial as to the result of the election. After studying the policies advocated by Brown and Green and the policies currently being pursued by the other four nations, each economist made a forecast. The forecast consisted of three predictions about the expected standard of living index (SLI). The SLI measures the goods and services consumed (directly or indirectly) by the average citizen yearly. It is expressed in dollars per capita so that the higher the SLI the higher the level of economic prosperity. The three projections concerned

1. the average SLI to be expected among the nations Beta, Gamma, Delta, and Epsilon
2. the SLI to be expected by following Brown's policy
3. the SLI to be expected by following Green's policy

The forecasts made by each economist are summarized in the following table:

	Projected SLI in Dollars per Capita		
	Other Four Nations	Brown's Policy	Green's Policy
Economist 1	$43,000	$65,000	$51,000
Economist 2	$45,000	$43,000	$53,000

Suppose that as a citizen of Alpha, you were asked to cast your vote for Brown or Green. On the basis of the information provided, whom would you vote for? [Brown, 28%; Green, 72%]

A second group of respondents received the same cover story as in Problem 1, but the economists' forecasts about the other four nations were altered. The forecasts made about the candidates remained the same.

Problem 2 (*N* = 96)

	Projected SLI in Dollars per Capita		
	Other Four Nations	Brown's Policy	Green's Policy
Economist 1	$63,000	$65,000	$51,000
Economist 2	$65,000	$43,000	$53,000

Suppose that as a citizen of Alpha, you were asked to cast your vote for Brown or Green. On the basis of the information provided, whom would you vote for? [Brown, 50%; Green, 50%]

Comparing the responses to problems 1 and 2 shows that the choice between Brown and Green was influenced by the projected SLI in other countries. This effect can be explained in terms of the value function of prospect theory. Because the two economists were said to be impartial and of equal expertise, we assume that respondents gave equal weight to their projections. Hence, the actuarial expected value of Brown's policy ($54,000) is about the same as that of Green's policy ($52,000). However, Brown is riskier than Green in the sense that the outcomes projected for Brown have greater spread than those projected for Green. Therefore, Brown would profit from risk seeking and Green from risk aversion. According to prospect theory, an individual's attitude towards risk depends on whether the outcomes are perceived as gains or losses, relative to the reference point.

In Problems 1 and 2 it seems reasonable to adopt the average SLI projected for the other nations as a point of reference, because all five nations were said to have comparable standards of living. The reference point then will be about \$44,000 in problem 1 and \$64,000 in problem 2. Outcomes projected for Brown and Green would, therefore, be treated as gains in the first problem and as losses in the second. As a consequence, the value function entails more risk aversion in problem 1 than in problem 2. In fact, significantly more respondents opted for the relatively risk-free Green in problem 1 (72%) than in problem 2 (50%) ($p < .005$ by chi-square). Another factor that may have contributed to the finding is a tendency for people to discount the highly discrepant projection for the risky candidate, Brown (i.e., the one made by Economist 1 in problem 1 and by Economist 2 in problem 2). Although this consideration may have played a role in the present case, the same shift in attitudes towards risk have been observed in many other problems in which this account does not apply (Tversky and Kahneman 1986).

To address whether the predictions based on the value function apply to other attributes besides money, we included in the same questionnaire one of two problems in which the rate of inflation was the outcome of the choice.

Problem 3 ($N = 76$)

> Now imagine that several years have passed and that there is another presidential contest between two new candidates, Frank and Carl. The same two economists studied the candidates' preferred policies and made a projection. This time, however, the forecast concerned the projected rate of inflation. The forecasts made by each economist are summarized in the following table:

	Projected Rate of Inflation (%)		
	Other Four Nations	Frank's Policy	Carl's Policy
Economist 1	24	16	4
Economist 2	26	14	26

> Suppose that as a citizen of Alpha, you were asked to cast your vote for Frank or Carl. On the basis of the information provided, whom would you vote for? [Frank, 74%; Carl, 26%]

A second group of respondents received the same cover story as in problem 3, but the economists' forecasts about the other four nations were altered. The forecasts made about the candidates remained the same.

Problem 4 ($N = 75$)

	Projected Rate of Inflation (%)		
	Other Four Nations	Frank's Policy	Carl's Policy
Economist 1	4	16	4
Economist 2	6	14	26

> Suppose that as a citizen of Alpha, you were asked to cast your vote for Frank or Carl. On the basis of the information provided, whom would you vote for? [Frank, 52%; Carl, 48%]

The analysis of problems 3 and 4 closely follows that of problems 1 and 2. The expected rate of inflation was 15% for both candidates. However, this value was below the expected continental rate of 25% in problem 3 and above the expected continental rate of 5% in problem 4. Because high inflation is undesirable, values below reference are likely to be viewed as gains, whereas values above reference are likely to be viewed as losses. Assuming that the continental rate of inflation was taken as a point of reference, the results confirmed the prediction of prospect theory that the more risky candidate (Carl) would obtain more votes in problem 4 (48%) than in problem 3 (26%) ($p < .01$ by chi-square).

Together, the responses to problems 1–4 confirm the prediction of prospect theory that people are risk-averse in the domain of gains and risk-seeking in the domain of losses, where gains and losses were defined relative to the outcomes projected for other countries. These results may shed light on the so-called incumbency-oriented voting hypothesis.

Numerous investigators have shown that the evaluation of an incumbent party is responsive to fluctuations in the national economy. In general, incumbent presidents and congressional candidates of the same party benefit at the polls from improving economic conditions whereas they suffer from deteriorating conditions (Kramer 1971). These results can be understood, in part, as a consequence of the divergent attitudes towards risks for outcomes involving gains and losses. Following Shepsle (1972), we maintain that incumbents are usually regarded by voters as less risky than the challengers, who are often unknowns and whose policies could drastically alter the current trends, for better or for worse. If people are risk-averse for gains and risk-seeking for losses, the less risky incumbent should fare better when conditions are good than when they are bad. This analysis assumes that the reelection of the incumbent is perceived by voters as a continuation of the current trends, which is attractive when times are good. In contrast, the election of the challenger offers a political gamble that is worth taking when "four more years" of the incumbent is viewed as an unsatisfactory state.

It is important to distinguish this analysis of incumbency-oriented voting from the more common explanation that "when times are bad you throw the rascals out." In the latter account, voters are thought to regard a credible challenger as having to be better than the incumbent, who "got us into this mess to begin with." The present account, in contrast, is based on the notion that the challenger is *riskier* than the incumbent, not necessarily better overall. In problems 2 and 4, the risky candidates profit from hard times even though their expected value was no better than that of the relatively riskless candidates. Obviously, however, a challenger whose expected value is substantially below the incumbent's is unlikely to be elected even in the presence of substantial risk seeking.

In light of this discussion, it is interesting to share an unsolicited response given by one of our participants, who received problem 4 in the winter of 1981. This respondent penciled in *Carter* over Frank, the less risky candidate, and *Reagan* over Carl, the riskier candidate. Recall that in this problem the outcomes were less desirable than the reference point. Evidently, our respondent—who voted for Carl—believed that the erstwhile incumbent Carter would have guaranteed the continuation of unacceptable economic conditions, while the erstwhile challenger Reagan, with his risky "new" theories, might have made matters twice as bad as they were or might have been able to restore conditions to a satisfactory level. Because economic and global conditions were widely regarded as unacceptable in 1980, the convexity of the value function for losses may have contributed to the election of a risky presidential prospect, namely Reagan.

Loss Aversion

A significant feature of the value function is that losses loom larger than gains. For example, the displeasure associated with losing a sum of money is generally greater than the pleasure associated with winning the same amount. This property, called *loss aversion*, is depicted in Figure 1 by the steeper slope for outcomes below the reference point than for those above.

An important consequence of loss aversion is a preference for the status quo over alternatives with the same expected value. For example, most people are reluctant to accept a bet that offers equal odds of winning and losing x number of dollars. This reluctance is consistent with loss aversion, which implies that the pain associated with the loss would exceed the pleasure associated with the gain, or $v(x) <$

$-v(-x)$. This observation, however, is also consistent with the concavity of the utility function, which implies that the status quo (i.e., the prospect yielding one's current level of wealth with certainty) is preferred to any risky prospect with the same expected value. These accounts can be discriminated from each other because in utility theory the greater impact of losses than of gains is tied to the presence of risk. In the present analysis, however, loss aversion also applies to riskless choice. Consider the following example: Let $x = (x_i, x_u)$ and $y = (y_i, y_u)$ denote two economic policies that produce inflation rates of x_i and y_i and unemployment rates of x_u and y_u. Suppose $x_i > y_i$ but $x_u < y_u$; that is, y produces a lower rate of inflation than x but at the price of a higher rate of unemployment. If people evaluate such policies as positive or negative changes relative to a neutral multiattribute reference point and if the (multiattribute) value function exhibits loss aversion, people will exhibit a reluctance to trade; that is, if at position x (the status quo) people are indifferent between x and y, then at position y they would not be willing to switch to x (Kahneman and Tversky 1984). We test this prediction in the following pair of problems.

Problem 5 ($N = 91$)

> Imagine there were a presidential contest between two candidates, Frank and Carl. Frank wishes to keep the level of inflation and unemployment at its current level. The rate of inflation is currently at 42%, and the rate of unemployment is currently at 15%. Carl proposes a policy that would decrease the rate of inflation by 19% while increasing the rate of unemployment by 7%. Suppose that as a citizen of Alpha, you were asked to cast your vote for either Frank or Carl. Please indicate your vote. [Frank, 65%; Carl, 35%]

Problem 6 ($N = 89$)

> Imagine there were a presidential contest between two candidates, Frank and Carl. Carl wishes to keep the rate of inflation and unemployment at its current level. The rate of inflation is currently at 23%, and the rate of unemployment is currently at 22%. Frank proposes a policy that would increase the rate of inflation by 19% while decreasing the rate of unemployment by 7%. Suppose that as a citizen of Alpha you were asked to cast your vote for either Frank or Carl. Please indicate your vote. [Frank, 39%; Carl, 61%]

It is easy to see that problems 5 and 6 offer the same choice between Frank's policy (42%, 15%) and Carl's policy (23%, 22%). The problems differ only in the location of the status quo, which coincides with Frank's policy in problem 5 and with Carl's policy in problem 6. As implied by the notion of multiattribute loss aversion, the majority choice in both problems favored the status quo ($p < .001$ by chi-square). The reluctance to trade is in this instance incompatible with standard utility theory, in which the preference between two policies should not depend on whether one or the other is designated as the status quo. In terms of a two-dimensional value function, defined on changes in inflation and unemployment, the present results imply that both $v(19, -7)$ and $v(-19, 7)$ are less than $v(0, 0) = 0$.

We have seen that the combination of risk aversion for gains and risk seeking for losses is consistent with incumbency-oriented voting: incumbents profit from good times, and challengers from bad times. We wish to point out that loss aversion is consistent with another widely accepted generalization, namely that the incumbent enjoys a distinct advantage over the challenger. This effect is frequently attributed to such advantages of holding office as that of obtaining free publicity while doing one's job and being perceived by voters as more experienced and effective at raising funds (Kiewiet 1982). To these considerations, the present analysis of choice adds the consequences of the value function. Because it is natural to take the incumbent's policy as the status quo—the reference point to which the challenger's policy is compared —and because losses loom larger than gains, it follows that the incumbent enjoys a distinct advantage. As we argued earlier, the introduction of risk or uncertainty also tends to favor the incumbent under conditions that enhance risk aversion; that is, when the general conditions are good or even acceptable, voters are likely to play it safe and opt for the relatively riskless incumbent. Only when conditions become unacceptable will the risky challenger capture an edge. Hence, the properties of the value function are consistent with the generally observed incumbency effects, as well as with the exceptions that are found during hard times.

Loss aversion may play an important role in bargaining and negotiation. The process of making compromises and concessions may be hindered by loss aversion, because each party may view its own concessions as losses that loom larger than the gains achieved by the concessions of the adversary (Bazerman 1983; Tversky and Kahneman 1986). In negotiating over missiles, for example, each superpower may sense a greater loss in security from the dismantling of its own missiles than it senses a gain in security from a comparable reduction made by the other side. This difficulty is further compounded by the fact, noted by several writers (e.g., Lebow and Stein 1987; Ross 1986), that the very willingness of one side to make a particular concession (e.g., eliminate missiles from a particular location) immediately reduces the perceived value of this concession.

An interesting example of the role of the reference point in the formation of public opinion was brought to our attention by the actor Alan Alda. The objective of the Equal Rights Amendment (ERA) can be framed in two essentially equivalent ways. On the one hand, the ERA can be presented as an attempt to eliminate discrimination against women. In this formulation, attention is drawn to the argument that equal rights for women are not currently guaranteed by the constitution, a negative state that the ERA is designed to undo. On the other hand, the ERA can be framed as legislation designed to improve women's status in society. This frame emphasizes what is to be gained from the amendment, namely, better status and equal rights for women. If losses loom larger than gains, then support for the ERA should be greater among those who are exposed to the frame that emphasizes the elimination of discrimination than the improvement of women's rights. To test Alda's hypothesis, we presented two groups of respondents with the following question. The questions presented to the two groups differed only in the statement appearing on either side of the slash within the brackets.

Problem 7 ($N = 149$)

> As you know, the Equal Rights Amendment to the Constitution is currently being debated across the country. It says, "Equality of rights under law shall not be denied or abridged by the United States or by any state on account of sex." Supporters of the amendment say that it will [help eliminate discrimination against women/ improve the rights of women] in job opportunities, salary, and social security benefits. Opponents of the amendment say that it will have a negative effect by denying women protection offered by special laws. Do you favor or oppose the Equal Rights Amendment? (check one)

Not surprisingly, a large majority of our sample of Stanford undergraduates indicated support for the ERA (74%). However, this support was greater when the problem was framed in terms of eliminating discrimination (78%) than in terms of improving women's rights (69%).

Just as the formulation of the issue may affect the attitude of the target audience, so might the prior attitude of the audience have an effect on the preferred formulation of the issue. Another group of respondents first indicated their opinion on the ERA, either pro or con. They then responded to the following question.

Problem 8 ($N = 421$)

> The status and rights of women have been addressed in two different ways, which have different social and legal implications. Some people view it primarily as a problem of eliminating inequity and discrimination against women in jobs, salary, etc. Other people view it primarily as a problem of improving or strengthening the rights of women in different areas of modern society. How do you see the problem of women's rights? (check one only)

Of those who indicated support of the ERA, 72% chose to frame the issue in terms of eliminating inequity, whereas only 60% of those who opposed the ERA chose this frame. This finding is consistent with the common observation regarding the political significance of how issues are labeled. A familiar example involves abortion, whose opponents call themselves prolife, not antichoice.

Invariance, Framing, and the Ratio-Difference Principle

Perhaps the most fundamental principle of rational choice is the assumption of invariance. This assumption, which is rarely stated explicitly, requires that the preference order among prospects should not depend on how their outcomes and probabilities are described and thus that two alternative formulations of the same problem should yield the same choice. The responses to problems 7 and 8 above may be construed as a failure of invariance. In the present section, we present sharper tests of invariance in which the two versions of a given choice problem are unquestionably equivalent. Under these conditions, violations of invariance cannot be justified on normative grounds. To illustrate such failures of invariance and motivate the psychological analysis, consider the following pair of problems.

Problem 9 ($N = 126$)

> Political decision making often involves a considerable number of trade-offs. A program that benefits one segment of the population may work to the disadvantage of another segment. Policies designed to lead to higher rates of employment frequently have an adverse effect on inflation. Imagine you were faced with the decision of adopting one of two economic policies.
>
> If program *J* is adopted, 10% of the work force would be unemployed, while the rate of inflation would be 12%. If program *K* is adopted, 5% of the work force would be unemployed, while the rate of inflation would be 17%. The following table summarizes the alternative policies and their likely consequences:

Policy	Work Force Unemployed (%)	Rate of Inflation (%)
Program *J*	10	12
Program *K*	5	17

> Imagine you were faced with the decision of adopting program *J* or program *K*. Which would you select? [program *J*, 36%; program *K*, 64%]

A second group of respondents received the same cover story about trade-offs with the following description of the alternative policies:

Problem 10 ($N = 133$)

Policy	Work Force Employed (%)	Rate of Inflation (%)
Program *J*	90	12
Program *K*	95	17

> Imagine you were faced with the decision of adopting program *J* or program *K*. Which would you select? [program *J*, 54%; program *K*, 46%]

The modal response was program *K* in problem 9 and program *J* in problem 10. These choices constitute a violation of invariance in that each program produces the same outcomes in both problems. After all, to say that 10% or 5% of the work force will be unemployed is to say, respectively, that 90% or 95% of the work force will be employed. Yet respondents showed more sensitivity to the outcomes when these were described as rates of unemployment than as rates of employment. These results illustrate a "psychophysical" effect that we call the *ratio-difference principle.*

Psychophysics is the study of the functional relation between the physical and the psychological value of attributes such as size, brightness, or loudness. A utility function for money, therefore, can also be viewed as a psychophysical scale relating the objective to the subjective value of money. Recall that a concave value function for gains of the form depicted in Figure 1 implies that a difference between $100 and $200 looms larger than the objectively equal difference between $200 and $300. More generally, the ratio-difference principle says that the impact of any fixed positive difference between two amounts increases with their ratio. Thus the difference between $200 and $100 yields a ratio of 2, whereas the difference between $300 and $200 yields a ratio of 1.5. The ratio-difference principle applies to many perceptual attributes. Increasing the illumination of a room by adding one candle has a much larger impact when the initial illumination is poor than when it is good. The same pattern is observed for many sensory attributes, and it appears that the same psychophysical principle is applicable to the perception of numerical differences as well.

Unlike perceptual dimensions, however, numerical scales can be framed in different ways. The labor statistics, for example, can be described in terms of employment or unemployment, yielding the same difference with very different ratios. If the ratio-difference principle applies to such scales, then the change from an unemployment rate of 10% to 5%, yielding a ratio of 2, should have more impact than the objectively equal change from an employment rate of 90% to 95%, yielding a ratio that is very close to unity. As a consequence, program *K* would be more popular in problem 9 and program *J* in problem 10. This reversal in preference was obtained, although the only difference between the two problems was the use of unemployment data in problem 9 and employment data in problem 10.

The ratio-difference principle has numerous applications to political behavior. For example, many political choices involve the allocation of limited funds to various sectors of the population. The following two problems demonstrate how the framing of official statistics can effect the perceived need for public assistance.

Problem 11 ($N = 125$)

The country of Delta is interested in reducing the crime rate among its immigrant groups. The Department of Justice has been allocated $100 million ($100M) for establishing a crime prevention program aimed at immigrant youths. The program would provide the youths with job opportunities and recreational facilities, inasmuch as criminal acts tend to be committed by unemployed youths who have little to do with their time. A decision must be made between two programs currently being considered. The programs differ from each other primarily in how the $100M would be distributed between Delta's two largest immigrant communities, the Alphans and the Betans. There are roughly the same number of Alphans and Betans in Delta. Statistics have shown that by the age of 25, 3.7% of all Alphans have a criminal record, whereas 1.2% of all Betans have a criminal record.

The following two programs are being considered. Program *J* would allocate to the Alphan community $55M and to the Betan community $45M. Program *K* would allocate $65M to the Alphan community and to the Betan community $35M. The following table summarizes these alternative programs:

Program	To Alphan Community	To Betan Community
Program *J*	$55M	$45M
Program *K*	$65M	$35M

Imagine you were faced with the decision between program *J* and program *K*. In light of the available crime statistics, which would you select? [program *J*, 41%; program *K*, 59%]

A second group of respondents received the same cover story and program description as in problem 11, with the criminal statistics framed as follows:

Problem 12 ($N = 126$)

> Statistics have shown that by the age of 25, 96.3% of all Alphans have no criminal record whereas 98.8% of all Betans have no criminal record. . . . In light of the available crime statistics, which would you select? [program *J*, 71%; program *K*, 29%]

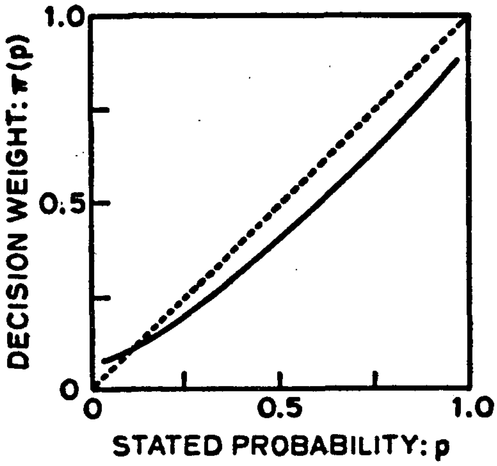
It should be apparent that the crime statistics on which respondents were to base their choice were the same across the two problems. Because of the ratio-difference principle, however, the Alphans are perceived as much more criminal than the Betans in problem 11—roughly three times as criminal—but they are seen as only slightly less noncriminal than the Betans in problem 12. As hypothesized, respondents selected that program in which differences in allocations between the groups matched as closely as possible differences in perceived criminality, resulting in a large reversal of preference ($p < .001$ by chi-square).

The preceding two problems illustrate an important social problem concerning the perception of crime rates among minority and nonminority segments of the population. It is generally believed that the members of minority groups, such as blacks, have much higher crime rates than do the members of nonminority groups, such as whites (Tursky et al. 1976). Indeed, according to the actual crime statistics compiled by the FBI in 1982, 2.76% of black citizens were arrested for a serious crime compared to .68% of white Americans. The between group difference does appear quite large. Problems 11 and 12 suggest, however, that judgments about the divergent crime rates in the two communities may be altered by how the data are framed. The apparently large difference between crime rates of 2.76% and .68% can be reframed as a relatively small difference between law-obedience rates of 97.24% and 99.32%.

Quattrone and Warren (1985) showed a sample of Stanford undergraduates the 1982 crime statistics, framed either in terms of the percentages of blacks and whites who were arrested for crime or the percentages who were not. Other respondents were not exposed to these data. As implied by the ratio-difference principle, the respondents who were exposed to the crime commission statistics considered the crime rate to be substantially higher in black communities than in white communities, whereas those exposed to the law-obedience statistics considered the communities to be more at par in crime. Furthermore, the subjects who were not shown the FBI crime data gave responses that were virtually indistinguishable from those given by subjects exposed to the crime commission statistics. This comparison suggests that people may generally formulate beliefs about the proportions of blacks and whites who commit crime, not the proportions who abide by the law.

In another question the subjects who had consulted the FBI statistics were asked to allocate $100M targeted for the prevention of crime between the two racial communities. It was observed that subjects exposed to the crime commission statistics allocated more money to the black community (mean = $58.4M) than did the subjects exposed to the law obedience statistics (mean = $47.2M). Hence, the basic results of this section were replicated for nonhypothetical groups. Moreover, a second study by Quattrone and Warren demonstrated that the same reversals due to framing are obtained when racial differences in crime must be inferred from a set of photographs rather than being explicitly pointed out in a neat statistical table. Taken as a whole, the results suggest that the decision of how to frame the data can have significant political conse-

Figure 2. A Hypothetical Weighting Function



quences for individuals as well as for entire social groups. We suspect that the more successful practitioners of the art of persuasion commonly employ such framing effects to their personal advantage.

The Weighting of Chance Events

A cornerstone of the rational theory of choice is the expectation principle. In the expected utility model, the decision maker selects that option with the highest expected utility that equals the sum of the utilities of the outcomes, each weighted by its probability. The following example of Zeckhauser illustrates a violation of this rule. Consider a game of Russian roulette where you are allowed to purchase the removal of one bullet. Would you be willing to pay the same amount to reduce the number of bullets from four to three as you would to reduce the number from one to zero? Most people say that they would pay more to reduce the probability of death from one-sixth to zero, thereby eliminating the risk altogether, than to reduce the probability of death from four-sixths to three-sixths. This response, however, is incompatible with the expectation principle, according to which the former reduction from a possibility (one bullet) to a certainty (no bullets) cannot be more valuable than the latter reduction (from four to three bullets). To accommodate this and other violations of the expectation principle, the value of each outcome in prospect theory is multiplied by a decision weight that is a monotonic but nonlinear function of its probability.

Consider a simple prospect that yields outcome x with probability p, outcome y with probability q, and the status quo with probability $1 - p - q$. With the reference point set at the status quo, the outcomes are assigned values $v(x)$ and $v(y)$, and the probabilities are assigned *decision weights*, $\pi(p)$ and $\pi(q)$. The overall value of the prospect is

$$\pi(p)v(x) + \pi(q)v(y).$$

As shown in Figure 2, π is a monotonic nonlinear function of p with the following properties:

1. Impossible events are discarded, that is, $\pi(0) = 0$, and the scale is normalized so that $\pi(1) = 1$. The function is not well behaved at the endpoints though, for people sometimes treat highly likely events as certain and highly unlikely events as impossible.
2. Low probabilities are overweighted, giving rise to some risk seeking in the domain of gains. For example, many people prefer one chance in a thousand to win \$3,000 over \$3 for sure. This implies

   $$\pi(.001)v(\$3{,}000) > v(\$3),$$

   hence

   $$\pi(.001) > v(\$3)/v(\$3{,}000) > .001,$$

   by the concavity of v for gains.

3. Although for low probabilities, $\pi(p) > p$, in general, $\pi(p) + \pi(1 - p) < 1$. Thus low probabilities are overweighted, moderate and high probabilities are underweighted, and the latter effect is more pronounced than the former.
4. For all $0 < p, q, r < 1$, $\pi(pq)/\pi(p) < \pi(pqr)/\pi(pr)$; that is, for any ratio of probabilities q, the ratio of decision weights is closer to unity when the probabilities are small than when they are large; for example, $\pi(.4)/\pi(.8) < \pi(.1)/\pi(.2)$. This property implies the common response to the Russian roulette problem because $\pi(1/6) - \pi(0) > \pi(4/6) - \pi(3/6)$.

Although the description of π has involved stated numerical probabilities, it can be extended to events whose probabilities are subjectively assessed or verbally implied. In these situations, however, the decision weights may also be affected by the vagueness or other details of the choice.

Certainty and Pseudocertainty

Many public policies involve the allocation of funds for projects whose outcomes cannot be known with certainty. The following problems illustrate how preferences among risky projects may be affected by the properties of π, and the results are contrasted with those predicted by the expected utility model.

Problem 13 ($N = 88$)

The state of Epsilon is interested in developing clean and safe alternative sources of energy. Its Department of Natural Resources is considering two programs for establishing solar energy within the state. If program *X* is adopted, then it is virtually certain that over the next four years the state will save $20 million ($20M) in energy expenditures. If program *Y* is adopted, then there is an 80% chance that the state will save $30M in energy expenditures over the next four years and a 20% chance that because of cost overruns, the program will produce no savings in energy expenditures at all. The following table summarizes the alternative policies and their probable consequences.

Policy	Savings in Energy Expenditures
Program *X*	$20M savings, with certainty
Program *Y*	80% chance of saving $30M, 20% chance of no savings

Imagine you were faced with the decision of adopting program *X* or program *Y*. Which would you select? [program *X*, 74%; program *Y*, 26%]

The same respondents who received problem 13 also received the following problem. Order of presenting the two problems was counterbalanced across booklets.

Problem 14 ($N = 88$)

The state of Gamma is also interested in developing clean and safe alternative sources of energy. Its Department of Natural Resources is considering two programs for establishing solar energy within the state. If program *A* is adopted, then there is a 25% chance that over the next four years the state will save $20 million ($20M) in energy expenditures and a 75% chance that because of cost overruns, the program will produce no savings in energy expenditures at all. If program *B* is adopted, there is a 20% chance that the state will save $30M in energy expenditures and an 80% chance that because of cost overruns, the program will produce no savings in energy expenditures at all. The following table summarizes the alternative policies and their probable consequences:

Policy	Savings in Energy Expenditures
Program *A*	25% chance of saving $20M, 75% chance of no savings
Program *B*	20% chance of saving $30M, 80% chance of no savings

Imagine you were faced with the decision of adopting program *A* or program *B*. Which would you select? [program *A*, 39%; program *B*, 61%]

Because the same respondents completed both problems 13 and 14, we can examine the number who selected each of the four possible pairs of programs: *X* and

A, *X* and *B*, *Y* and *A*, *Y* and *B*. These data are shown in below.

	Problem 14	
Problem 13	Program *A*	Program *B*
Program *X*	27	38
Program *Y*	7	16

The pair most frequently selected is *X* and *B*, which corresponds to the modal choices of each problem considered individually. These modal choices pose a problem for the expected utility model. Setting $u(0) = 0$, the preference for *X* over *Y* in problem 13 implies that $u(\$20M) > (4/5)u(\$30M)$, or that $u(\$20M)/u(\$30M) > 4/5$. This inequality is inconsistent with that implied by problem 14, because the preference for *A* over *B* implies that $(1/4)u(\$20M) < (1/5)u(\$30M)$, or that $u(\$20M)/u(\$30M) < 4/5$. Note that programs *A* and *B* (in problem 14) can be obtained from programs *X* and *Y* (in problem 13), respectively, by multiplying the probability of nonnull outcomes by one-fourth. The substitution axiom of expected utility theory says that if *X* is preferred to *Y*, then a probability mixture that yields *X* with probability p and 0 otherwise should be preferred to a mixture that yields *Y* with probability p and 0 otherwise. If $p = 1/4$, this axiom implies that *X* is preferred to *Y* if and only if *A* is preferred to *B*. From the above table it is evident that more than half of our respondents (45 or 88) violated this axiom.

The modal choices, *X* and *B*, however, are consistent with prospect theory. Applying the equation of prospect theory to the modal choice of problem 13 yields $\pi(1)v(\$20M) > \pi(.8)v(\$30M)$, hence $v(\$20M)/v(\$30M) > \pi(.8)/\pi(1)$. Applied to problem 14, the equation yields $\pi(.2)/\pi(.25) > v(\$20M)/v(\$30M)$. Taken together, these inequalities imply the observed violation of the substitution axiom for those individuals for which $\pi(.8)/\pi(1) < v(\$20M)/v(\$30M) < \pi(.2)/\pi(.25)$. Recall that for any ratio of probabilities $q < 1$, the ratio of decision weights is closer to unity when the probabilities are small than when they are large. In particular, $\pi(.8)/\pi(1) < \pi(.2)/\pi(.25)$. Indeed, 38 of the 45 pairs of choices that deviate from expected utility theory fit the above pattern, $p < .001$ by sign test.

It should be noted that prospect theory does not predict that all respondents will prefer *X* to *Y* and *B* to *A*. This pattern will be found only among those respondents for whom the value ratio, $v(\$20M)/v(\$30M)$, lies between the ratios of decision weights, $\pi(.8)/\pi(1)$ and $\pi(.2)/\pi(.25)$. The theory requires only that individuals who are indifferent between *X* and *Y* will prefer *B* to *A* and those who are indifferent between *A* and *B* will prefer *X* to *Y*. For group data, the theory does predict the observed shift in modal preferences. The only pair of choices *not* consistent with prospect theory is *Y* and *A*, for this pair implies that $\pi(.2)/\pi(.25) < \pi(.8)/\pi(1)$. This pair was in fact selected least often.

The modal preferences exhibited in the preceding two problems illustrate a phenomenon first reported by Allais (1953) that is referred to in prospect theory as the *certainty effect:* reducing the probability of an outcome by a constant factor has a greater impact when the outcome was initially certain than when it was merely possible. The Russian roulette game discussed earlier is a variant of the certainty effect.

Causal versus Diagnostic Contingencies

A classical problem in the analysis of political behavior concerns the rationality of voting and abstaining. According to Downs (1957), it may not be rational for an individual to register and vote in large elections because of the very low probability that the individual would cast a decisive vote coupled with the costs of registering and going to the polls. Objections to Downs's view were raised by Riker and Ordeshook (1968), who argued that an individual may derive from voting other benefits besides the possibility of casting a decisive ballot. These additional benefits are collectively referred to as *citizen's duty*, or *D*, and they include affirming one's allegiance to the democratic system, complying with a powerful ethic, participating in a common social ritual, as well as "standing up and being counted." To these rational consequences of voting, we suggest adding a somewhat less rational component.

Elsewhere (Quattrone and Tversky 1984) we have shown that people often fail to distinguish between causal contingencies (acts that produce an outcome) and diagnostic contingencies (acts that are merely correlated with an outcome). For example, there is a widespread belief that attitudes are correlated with actions. Therefore, some people may reason that if they decide to vote, that decision would imply that others with similar political attitudes would also decide to vote. Similarly, they may reason that if they decide to abstain, others who share their political attitudes will also abstain. Because the preferred candidates can defeat the opposition only if politically like-minded citizens vote in greater numbers than do politically unlike-minded citizens, the individual may infer that he or she had better vote; that is, each citizen may regard his or her single vote as diagnostic of *millions* of votes, which would substantially inflate the subjective probability of one's vote making a difference.

To test this hypothesis, which we call the *voter's illusion*, we had a sample of 315 Stanford undergraduates read about an imaginary country named Delta. Participants were to imagine that they supported party *A*, opposed party *B*, and that there were roughly four million supporters of each party in Delta as well as four million nonaligned voters. Subjects imagined that they were deliberating over whether to vote in the upcoming presidential election, having learned that voting in Delta can be costly in time and effort. To facilitate their decision, they were to consult one of two prevailing theories concerning the group of voters who would determine the electoral outcome.

Some subjects considered the *party supporter's theory*. According to this theory, the nonaligned voters would split their vote fairly equally across the two parties. The electoral outcome would be determined by whether the supporters of party *A* or party *B* became more involved in the election. The political experts were split as to whether the supporters of *A* or *B* would become more involved, but all agreed that the party whose members did become more involved would win by a margin of roughly 200 thousand to 400 thousand votes. Other subjects received the *nonaligned voter's theory*, which held that the supporters of each party would vote in equal numbers. The electoral outcome would in this account be determined by whether the nonaligned voters would swing their support primarily to party *A* or party *B*. The experts were split as to which party would capture the majority of the nonaligned voters, but all agreed that the fortunate party would win by a margin of at least 200 thousand votes.

Note that the consequences of voting included in the rational analysis are held constant across the two theories. In both, the "utility difference" between the two parties, the "probability" of casting a decisive vote, the costs of voting, and citizen's duty are the same. But according to the party supporter's theory, there is a correlation between political orientation and participation; that is, either the supporters of party *A* will vote in greater numbers than will the supporters of party *B*, or vice versa. In contrast, the nonaligned voter's theory holds that political orientation is independent of participation because party supporters will turn out in equal numbers. Therefore, only subjects presented with the former theory could infer that their decision to vote or to abstain would be diagnostic of what their politically like-minded peers would decide. If being able to make this inference is conducive to voting, then a larger "turnout" should be found among subjects presented with the party supporter's theory than among those presented with the nonaligned voter's theory. In fact, when asked, "Would you vote if the theory were true and voting in Delta were costly," significantly more subjects responded *no* under the party supporter's theory (16%) than under the nonaligned voter's theory (7%) ($p < .05$ by sign test).

An additional finding corroborated the analysis that this difference in turnout was attributable to the perceived diagnosticity of voting. Respondents were asked to indicate how likely it was that the supporters of party *A* would vote in greater numbers than the supporters of party *B* "given that you decided to vote" and "given that you decided to abstain." Responses to these two questions were made on nine-point scales with verbal labels ranging from "extremely likely" to "extremely unlikely." Subjects were informed that their decision to vote or abstain could not be communicated to others. Nonetheless, subjects exposed to the party supporter's theory thought that their individual choice would have a greater "effect" on what others decided to do than did subjects exposed to the nonaligned voter's theory, $F(1,313) = 35.79$ ($p < .001$). Similar effects were observed in responses to a question probing how likely party *A* was to defeat party *B* "given that you decided to vote" and "given that you decided to abstain," $F(1,313) = 40.18$ ($p < .001$). This latter difference was obtained despite subject's knowing that they could cast but one vote and that the likely margin of victory was about 200 thousand votes.

The observed differences between respondents exposed to the party supporter's and nonaligned voter's theory cannot be readily justified from a normative perspective (cf. Meehl 1977). The present analysis of causal versus diagnostic contingencies recalls the tragedy of the commons and it applies to other phenomena in which collective action dwarfs the causal significance of a single individual's contribution. The outcomes of most wars would not have changed had one fewer draftee been inducted, and the success or failure of most charity drives do not ordinarily depend on the dollars of an individual donor. These collective actions defy a routine rational analysis for the individual because if each citizen, draftee, or donor "rationally" refrains from making his or her paltry contribution, then the outcomes would be drastically affected. For this reason, exhortations to vote, to fight, and to help those less fortunate than oneself are usually framed, "If you don't vote/fight/contribute, think of what would happen if *everyone* felt the same way." This argument is compelling. Still, just how *does* an individual's private decision materially affect the decisions made by countless other persons?

Concluding Remarks

We contrasted the rational analysis of political decision making with a psychological account based on descriptive considerations. Although there is no universally accepted definition of rationality, most social scientists agree that rational choice should conform to a few elementary requirements. Foremost among these is the criterion of invariance (or extensionality [Arrow 1982]), which holds that the preference order among prospects should not depend on how they are described. Hence, no acceptable rational theory would allow reversals of preference to come about as a consequence of whether the choice is based on rates of employment or rates of unemployment, crime commission statistics or law obedience statistics. These alternate formulations of the problems convey the same information, and the problems differ from each other in no other way. We have seen, however, that these alternate frames led to predictable reversals in preference.

Whether our studies paint a humbling of flattering picture of human intellectual performance depends on the background from which they are viewed. The proponent of the rational theory of choice may find that we have focused on human limitations and have overlooked its many accomplishments. The motivational psychologist, accustomed to finding the root of all folly in deep-seated emotional needs, may find our approach much too rational and cognitive. Many readers are no doubt familiar with the versions of these opposing viewpoints found in political science. *The Authoritarian Personality* (Adorno et al. 1950), for example, well illustrates the use of motivational assumptions to explain the appeal of a particular ideology to certain elements of the population.

The descriptive failure of normative principles, such as invariance and coherence, does not mean that people are unintelligent or irrational. The failure merely indicates that judgment and choice—like perception and memory—are prone to distortion and error. The significance of the results stems from the observation that the errors are common and systematic, rather than idiosyncratic or random, hence they cannot be dismissed as noise. Accordingly, there is little hope for a theory of choice that is both normatively acceptable and descriptively adequate. A compelling analysis of the uses and abuses of rationality in theories of political behavior has been presented by Converse (1975) who has detailed the often arbitrary and inconsistent criteria by which rationality has been defined. Our intention was not to reopen the discussion about the meaning of rationality but rather to enrich the set of concepts and principles that could be used to analyze, explain, and predict the decisions made by individuals in their private lives, in the market place, and in the political arena.

Note

The research reported in this article was funded by a grant awarded to Quattrone by the National Institute of Health 1 RO1 MH41382-01 and to Tversky by the Office of Naval Research ON00014-84-K-0615. We are indebted to Philip Converse, Robyn Dawes, Alexander George, Robert Jervis, and Scott Plous for their helpful comments on an earlier draft.

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THE STRUCTURE OF EMOTIONAL RESPONSE: 1984 PRESIDENTIAL CANDIDATES

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Over the past two decades psychological models of affect have changed from valence (one-dimensional) models to multiple-dimensional models. The most recent models, circumplex models, are two-dimensional. Feeling thermometer measures, which derive their theoretical logic from earlier (valence) models of emotional appraisal, are shown to be confounded. Underlying the variation obtained using feeling thermometer measures are two dimensions of emotional response, mastery (positive emotionality) and threat (negative emotionality). Analysis of the 1984 NES survey suggests that positive emotional response is twice as influential as negative emotional response in predicting presidential candidate vote disposition to the presidential candidates. Reliance on emotional response is shown to be uniformly influential across various strata of the electorate.

Policy considerations have little direct influence on vote disposition, though policy considerations are indirectly related to vote disposition through the influence of issues on the degree of feelings of threat evoked by the candidates.

It has been 15 years since it was first recognized that asking people about their feelings toward presidential candidates was equivalent to ascertaining their vote disposition (Brody and Page 1973). Only recently have feelings—emotions—begun to be the subject of serious study by political scientists. The recent studies on emotions and politics have shown that feelings have strong predictive effects on candidate appraisal (Abelson et al. 1982), on issue appraisal (Conover and Feldman 1986), on political participation (Marcus 1985) and on political communication (Roseman, Abelson, and Ewing 1986). The primary concern of the initial studies has been to establish that emotions are worthy of study and that they add explanatory power to our understanding of a variety of political phenomena.[1]

While the studies cited above have established the value of studying emotions and in redressing the imbalance resulting from the dominance of the cognitive approach to human behavior, it must also be noted that these studies have failed to provide a sound theoretical focus. The recent attention to affect has been marked by contradictory and anomalous findings; so that even when the correct structure of emotional response has been identified, its significance and relevance have not been properly understood (e.g., Abelson et al. 1982). Contradictory findings have been reported in different studies as to the number of dimensions of emotionality. In a study of political communications, Roseman, Abelson, and Ewing (1986) report four dimensions. In a study of economic issues, Conover and Feldman (1986) report three dimensions. In a study of candidate appraisal, Abelson and his colleagues (1982) report two dimensions. And a recent study of the vote decision, Rahn and colleagues n.d., relies on one dimension of affect evaluation.

The anomalous result perplexing investigators has been the consistent finding that when individuals regard a situation or event (such as a candidate, issue, or circumstance) they often simultaneously report good and bad feelings (Diener and Emmons 1984; Warr, Barter, and Brownbridge 1983). Abelson and his colleagues (1982) find, after examining the emotional responses to various candidates for national office, that "perhaps our most psychologically provocative result is the near independence of negative and positive affect" (i.e., the frequent concurrent reports of both good and bad feelings). All of the studies that use multiple dimensions of emotionality report similar "anomalous" findings. Until and unless the structure of emotional response is clarified, how feelings interact with thinking and influence political behavior will remain obscured.

Three classes of models of the structure of affect are generally used: valence models, discrete models and circumplex models. Valence models are the basis of the familiar "thermometer scales" in the National Election Studies surveys. Discrete models are the basis of the feeling measures in the NES. Circumplex models have been a recent development and will be unfamiliar to most political scientists. Circumplex models offer a theoretical approach to variation in feelings that resolves the conflicting and anomalous findings. More importantly, the theory of circumplex models of affect suggests that feelings provide stable strategic interpretations and that variations in these strategic responses have important political implications.

Before turning to circumplex models of affect, it will be useful briefly to review the theoretical rationale of valence and discrete models of affect.

Valence Models of Emotion

In this approach to the study of beliefs and attitudes affect is defined as the *evaluative* dimension of an attitude, hence the term *valence.* Attitudes and beliefs are held about some object (e.g., person or issue). Attitudes are composed of three dimensions: affect, behavioral disposition, and cognition. Affect and evaluation are treated as synonymous (Fishbein and Ajzen 1975). Each attitude subject is evaluated positively *or* negatively *or* neutrally. The assumption underlying this class of models is that feelings are unidimensional and affectively bipolar (i.e., positive feelings at one end, negative feelings at the other, and neutral feelings in the middle). That some may evaluate an object positively while others are neutral or negative towards the object is to be understood as a matter of individual differences—an empirical matter to be studied. The tripartite conception of attitudes is gradually being abandoned by psychologists, because emotion and cognition are coming to be recognized as separate mental processes. It retains appeal to some (Breckler 1984).

In political science, feeling thermometers are the most frequent application of valence models of affect. The feeling thermometers have been a popular measure of assessing feelings elicited by candidates and by groups, parties, organizations, and economic classes. The 1984 NES lists 50 feeling thermometers, including the 18 feeling thermometers on political figures. The validity of findings that rely on feeling thermometers is dependent upon the assumption that affect has a bipolar one-dimensional structure. As this study will demonstrate, that assumption is invalid.

Discrete Models of Emotion

Discrete models of emotions share certain presumptions. First, different emotions are identified as "alternative appraisals of events" (Roseman 1984). The number of fundamental emotions differs from one model to another. Davies (1980) identifies seven principal affects: moral indignation, anger or hatred, envy or resentment, fear or suspicion, powerlessness or disillusion, sympathy or pity, and loyalty or pride. Izard (1971, 1977) proposed a 10-fundamental-emotions model. Roseman has proposed 13 fundamental emotions that characterize the structure of emotions (Roseman 1979). His most recent list of the fundamental emotions includes 14 fundamental emotions: joy, relief, hope, fear, frustration, liking, disliking, anger, guilt, pride, and regret (Roseman 1984). Different combinations of motivational states and situational factors present different circumstances, which then, in turn, elicit different interpretations (cognition) that produce an appropriate emotion (cognitively triggered affect). Perceptions trigger emotional responses depending on the motivational circumstance (for example, whether the event is perceived as rewarding or punishing) and on the situational context (for example, whether the event is seen as personally invested or personally neutral). A situation sensed as punitive will be experienced as a negative emotion: fear or distress if control of the situation is attributed to oneself, anger or hatred if control is attributed to someone else.

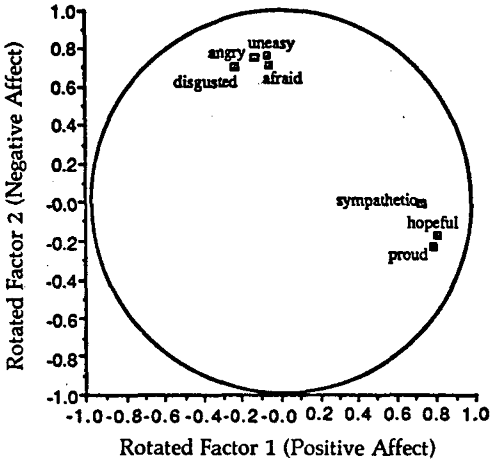
Discrete models have made some useful contributions, principally in beginning to pursue parsimony in the structure of emotional appraisal, particularly important since the number of distinguishable emotional terms is measured in the hundreds (e.g., Storm and Storm 1987; Wiener, Russell, and Lerman 1978, vol. 2). In particular, discrete models posit that evaluation (emotional appraisal) is to be understood in more strategic terms by positing a link between specific adaptive benefit (Pluthik 1980). Further, by emphasizing the emotional content of the representation of sensory stimuli and its influence on thought and action, discrete models have begun the transition from the orthodox position that cognitive processes can provide a comprehensive account of thinking, awareness, and behavior.[2]

However, proponents of discrete models disagree at the theoretical level as to the number of discrete emotions (ranging from 7 to 14); and at the empirical level, studies of politics using discrete models report varying dimensions of emotional appraisal (ranging from 2 to 4). And, of greatest importance, empirical studies consistently report that individuals report both positive and negative feelings so frequently that discrete models must conclude either that this is an anomalous finding or that most of the time people are in conflicted motivational states.

Circumplex Models of Emotion

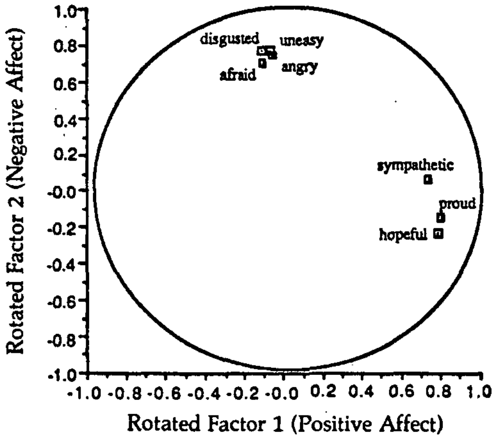
More recent developments have identified the structure of affect as a two-dimensional circumplex (Plutchik 1980; Russell 1980; Zevon and Tellegen 1982). The two dimensions are generally identified as positive emotionality and negative emotionality (affectively unipolar axes at orthogonal locations). Each dimension represents a specific arousal system. One arousal system characterizes certain motor-sensory signals as positive emotionality, ranging from dull and depressed when arousal is low and to elated enthusiastic when arousal is high. The second arousal system characterizes certain motor-sensory signals as negative emotionality, ranging from calm and placid

Figure 1. Emotional Response to Mondale on Seven Mood Terms



when arousal is low to nervous and anxious when arousal is high. The circumplex model locates all affect terms within the circumplex, the location varying as a result of the differential degrees of arousal on each dimension. Studies of facial expression, tones of voice, and affect words—in English and in other languages—all report the same basic two-dimensional structure (studies reviewed in Watson and Tellegen 1985; see also Masters and Sullivan 1986).

Figure 2. Emotional Response to Reagan on Seven Mood Terms

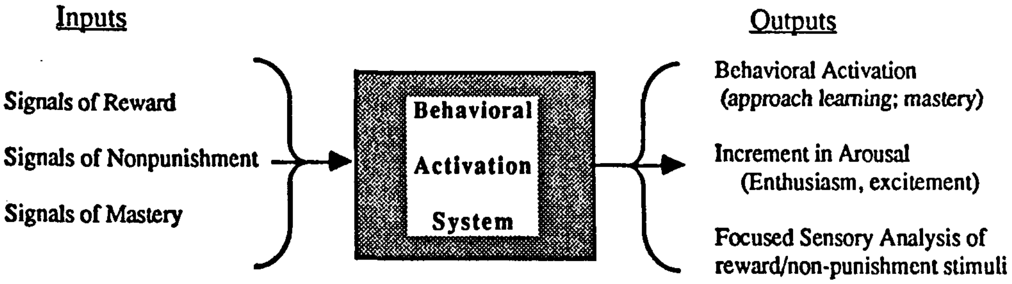


The circumplex model resolves why different studies have reported more than two dimensions. Studies that include affect terms that are heavily clustered in different regions of the circumplex and rely on oblique solutions will indeed report three or more dimensions (e.g., Conover and Feldman 1986; Roseman, Abelson, and Ewing 1986). However, reanalysis of studies with multiple oblique factors reports results that unambiguously support the two-dimensional circumplex model (Watson and Tellegen 1985). The Abelson and colleagues 1982 study of emotional appraisal of candidates, designed to demonstrate the relevance of emotions using one discrete model of emotions (Roseman's structural model; see Roseman 1979, 1984), reports results that validate the circumplex model (though they do not recognize it as such). Thus studies designed from a discrete model perspective, using measures intended to demonstrate the distinct influences of each primary emotion, have found, to the contrary, two dimensions of affect—positive and negative.

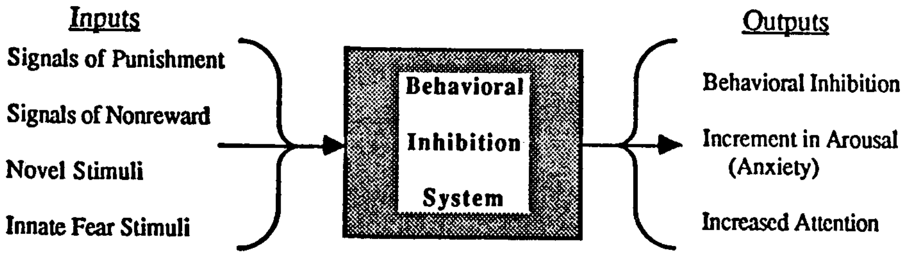
Preliminary Empirical Test

The 1984 NES data afford an opportunity to compare the circumplex model of emotion against valence and discrete models as it applies to emotional response to presidential candidates. Replicating the methodology used in previous NES studies, respondents in the 1984 NES were asked to regard the presidential candidates and asked if "something about that person, or something he has done has made you have certain feelings like anger or pride." Subjects then reported whether they have ever felt that emotion, using a list of seven discrete emotions selected from Roseman's discrete model of the structure of emotions (angry, hopeful,

Figure 3. Gray-Tellegen Two-Factor Model of Emotional Appraisal


Inputs
Signals of Reward
Signals of Nonpunishment
Signals of Mastery
Behavioral
Activation
System
Outputs
Behavioral Activation
(approach learning; mastery)
Increment in Arousal
(Enthusiasm, excitement)
Focused Sensory Analysis of
reward/non-punishment stimuli

The BAS System—Appraisal and Evaluation of Task Performance
(Scanning for Indications of Success/Failure of Subject Engaged Actions)


Inputs
Signals of Punishment
Signals of Nonreward
Novel Stimuli
Innate Fear Stimuli
Behavioral
Inhibition
System
Outputs
Behavioral Inhibition
Increment in Arousal
(Anxiety)
Increased Attention

The BIS System—Appraisal and Evaluation of Intrusive Information
(Scanning for External Danger or Threat)

afraid of him, proud, disgusted, sympathetic towards him, uneasy). This analysis should provide a robust test since the selection of the affect terms was intended to support the discrete model of emotions (Abelson et al. 1982; Roseman, Abelson, and Ewing 1986). Discrete models of emotions should find a multidimensional solution, probably oblique in structure. Figures 1 and 2 display plots of the factor loadings from factor analyses of the seven emotional measures for Mondale and for Reagan.

Only two dimensions emerge from the factor analysis.[3] The plotted factor loadings replicate the findings reported in Abelson *et alii* 1982; emotional response to presidential candidates is two-dimensional. These results contradict the expectations of the valence model that affective evaluation is one-dimensional and validates the circumplex model's expectation that affect is two-dimensional. The only departure from the circumplex model is the somewhat oblique relationship between the two dimensions (which will be addressed more fully in the later covariance analysis). These results are not specific to the 1984 NES data; comparable findings are reported in Abelson and colleagues 1982 and Sullivan and Masters 1988.

The discrete model, as well as a lay grasp of the labels *positive* and *negative,* would suggest that people feel either positive *or* negative feelings, but not both (i.e., affect dimensions would be bipolar rather than unipolar). The terms *positive* and *negative* seem mutually exclusive; much like *black* or *white, up* or *down.* Yet this is clearly not the case. To resolve this dilemma we need to turn to a more specific circumplex model of affect.

The personality models of Gray (1970, 1973, 1981) and Tellegen (1985) identify emotionality as the manifestation of two primary systems of emotional arousal.[4] Gray calls the first system the behavioral activation system (BAS). Tellegen calls it positive emotionality. The second system in Gray's model is the behavioral inhibition system (BIS), which Tellegen calls negative emotionality. Figure 3 depicts the Gray-Tellegen model of affect.

The primary contribution of the Gray-Tellegen model of emotion is that each system of emotional response is identified as having a distinct strategic function. One system is oriented towards appraising the implementation of subject-engaged action (positive emotionality). The strategic focus of the positive dimension of emotionality is self-regarding and task management.[5] This system monitors the ongoing mastery and control achieved by the individual as a result of the recruitment and allocation of mental and physical resources. This system produces variation in the arousal of positive emotion, affects of enthusiasm, pleasure, and excitement. Such affects occur when the task at hand is going well (and conversely, when things are going badly, affects of depression are likely). The second system, negative emotionality, monitors the environment for signs of evident danger. This system is environment-regarding; it is concerned with scanning for threats that may intrude and disrupt ongoing subject-engaged action. Some examples of such stimuli—loud or sudden noises or the appearance of a danger in the form of sudden bad weather during an airplane flight or an unexpectedly violent individual or hostile person appearing on the scene—reflect the kinds of stimuli that may elicit negative affects.

The two systems efficiently balance the assignment of mental and physical resources between self-absorption (full allocation to the task, or social activity, at hand, which is adaptive and efficient *if* anxiety is low—i.e., a safe environment) and the claims of attention warranted by external circumstances when confronting a threat (Simon 1967). Further, the two systems of emotionality are well adapted to maintaining social integration through emotional display (Masters and Sullivan 1986; Sullivan and Masters 1988). The development of models of affect has been a steady trend moving from a recognition that affect is engaged in evaluation (valence models) to the further recognition that affect is also linked to motivation (discrete models) and, more recently, to the recognition that affect supports two specific strategic evaluations (circumplex models).

The identification of two principal sources of behavior, self-initiating or self-engaged action on the one hand and action initiated in response to external causes on the other hand, is a familiar one in political science. For example, political efficacy is currently defined as two-dimensional (Craig and Maggiotto 1982; Shingles 1986). One dimension concerns attitudes toward self-initiating political action (internal political efficacy) and one dimension concerns attitudes assessing actions by others (external political efficacy). In psychology, a number of personality theories depend on this distinction. For example, recent work on self-monitoring (Snyder and Ickes 1985) and on field dependence also focus on the location of mastery (self versus situation) and the degree of mastery (high or low). Those described as high self-monitors rely on situational information to regulate their behavior. Those described as low self-monitors rely on information from relevant inner states to regulate their behavior. The locus of control literature (Phares 1978; Rotter 1966) also identifies two personality types on the basis of the issue of mastery: contrasting between those who believe that outcomes are the result of their own behavior (internals) and those who believe that outcomes are essentially contingent on luck, fate, or powerful others (externals).

The Gray-Tellegen model offers some advantages over these conceptions, though it has much that it shares with them. First, it distinguishes between the two dimensions, threat and mastery, and the degree of arousal (high or low) on each dimension. Second, it differentiates between mood and temperament (again enabling situational and dispositional influences to contend).[6] Third, it establishes connections between emotional response and motivation (something that information-processing models of cognition fail to do [Hastie 1986]). Finally, while it is almost certain that the Gray-Tellegen model is incomplete in important ways (Derryberry and Rothbart 1984), its identification of two fundamental superordinate dimensions is parsimonious and capable of emendation in light of further work.

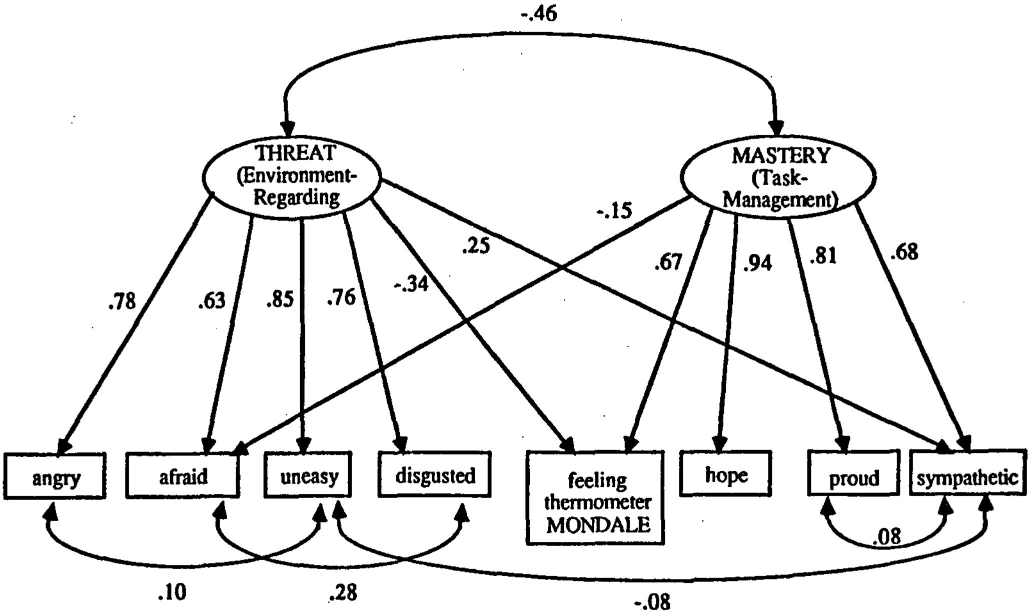
With respect to emotional responses to politicians, the Gray-Tellegen model also has specific advantages. The two dimensions of emotional response suggest that political candidates are appraised on two fundamental criteria. First, Is this candidate capable of being in personal command and able to make the environment more tractable, predictable, and bountiful, assuming that such resources are being used to achieve policies and actions supported by the individual (mastery)? The more these features are apparent, the greater the enthusiasm. Second, Is the candidate likely to pursue policies and actions not supported by the individual or give evidence of lack of stature or moral weakness (threat)? The more these features are apparent, especially if they become so suddenly or unexpectedly, the greater the anxiety and distress.

The strategic significance of these appraisals suggests why emotional responses to candidates, and other political subjects and events, are likely to have such empirical predictive validity. Helplessness and depression are inextricably intertwined (Seligman 1975) and concerns for mastery, usually couched as autonomy in political thought, have been central to liberal democratic theory (Bay 1965; Fromm 1941; Lane 1978; Mill 1956). It would be surprising indeed if information about candidates as to the enhancement or diminishing of mastery or the increase or decrease of threat did not play a central role in influencing support for political leaders.

Data Analysis

Fitting a confirmatory factor analysis (LISREL) model (Jöreskog 1969, 1970, 1973) to the 1984 NES data is an effective way of assessing whether the circumplex model can provide a superior alternative to discrete and valence model interpretations of the candidate feeling measures. LISREL requires that a model be specified on theoretical grounds. The LISREL program evaluates the goodness of fit, identifying where the model and data do and do not fit. The data consist of the covariance structure describing the relationships between the various feeling measures and the feeling thermometers. The variables are treated as manifest indicators of latent constructs. The Gray-Tellegen circumplex model identifies the proper structure of emotions as two dimensions, mastery and threat, each affectively unipolar, and the relationship between the two dimensions as orthogonal. Valence models would posit a single affectively bipolar dimension and discrete models would posit a multidimensional solution of 10, 13, or 14 oblique dimensions (limited in this in-

Figure 4. Two-Factor Model of Emotional Response: Mondale



Source: 1984 NES.

stance to 7 by the number of measures available in the NES).

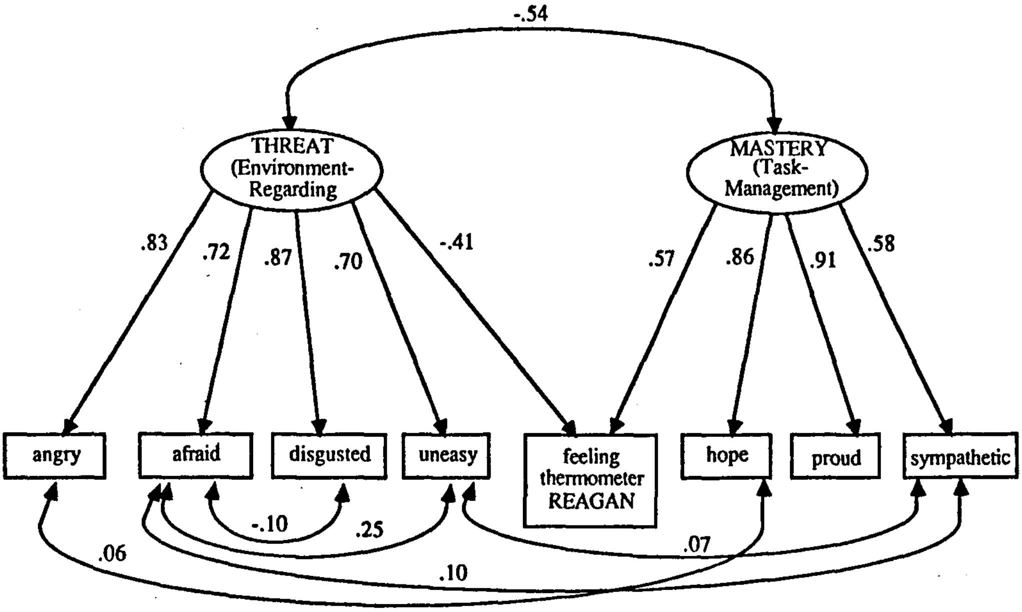
The latent construct *mastery* is defined by the indicators *hopeful, proud,* and *sympathetic*. The latent construct *threat* is defined by the indicators *angry, afraid, disgusted,* and *uneasy*. The feeling thermometers for Reagan and Mondale are identified as confounded measures; that is, the feeling thermometers are indicators that are causally dependent upon both latent constructs.

The model is estimated using polychoric and polyserial correlations, calculated by LISREL, because the feelings measures reported for each candidate are dichotomous. As a result the models are estimated using unweighted least squares (ULS).[7] Figures 4 and 5 present this model specification and the standardized solutions for Mondale and Reagan.

The chi-square value is not available for ULS estimation; however, goodness-of-fit indexes and, more importantly, the root mean square residual value are provided. For Mondale, the goodness-of-fit index is .999, the adjusted goodness of fit is also .999, and the root mean square residual is .013. For Reagan, the goodness-of-fit index is .999, the adjusted goodness-of-fit is .998, and the root mean square residual is .014. These values demonstrate that each model fits the data very well.

The variance in feeling thermometers is essentially defined by the two latent constructs of emotional response, threat and mastery. The model accounts for much of the variance of the feeling thermometers; $R^2 = .78$ for Mondale's feeling thermometer and $R^2 = .74$ for Reagan's feeling thermometer. These *R*-squared values are as high as are generally reported for single items in measurement models (the

Figure 5. Two-Factor Model of Emotional Response: Reagan



Source: 1984 NES.

remaining variance is assumed to be error variance; few single items are completely reliable indicators of a latent construct).

There are some correlated errors in each model, none from one domain to the other domain of great magnitude (the highest being $r = .10$ from *afraid* to *sympathetic* in the Reagan model). The factor analysis, reported in Figures 1 and 2, suggested that *sympathy* is more problematic as a measure of mastery (positive affect). The relatively low epistemic correlations (reliabilities) in each model and the epistemic relationship from threat to *sympathy* in the Mondale model confirm that assessment. Tomkins (1981) has suggested that some affective terms are combinations of biological primary affects and cognitive references. The LISREL and factor analysis of *sympathy* suggests that it may be one of these combinations.[8] The substantial negative correlations between the two latent factors, threat and mastery, may be due to either the choice of affect terms (see Appendix) or perhaps to the propensity for respondents to positively respond to political candidates (Lippmann 1922).

These results provide the preliminary evidence that using the feeling thermometer measures, even as a summary, "on-balance" measure of affect evaluation, obscures more than it reveals. Mastery plays a more influential role than threat in defining the feelings towards Mondale and Reagan. The epistemic correlations suggest that mastery may be as much as twice as influential as threat (an interpretation consistent with the reported factor analysis).[9] This suggests that identification with the candidate (positive affect) is the more potent consideration influencing the disposition to vote for a candidate—though threat is also a powerful influence (not surprising in the light of the frequent use of "negative campaigning").

But before turning to the question of how feelings may influence voting, can more evidence be brought forward to validate the task management–environment regarding distinction? Theories of democratic leadership identify at least two major ideal types of leadership roles, trustee and delegate (Pitkin 1967). The trustee role, as an ideal type, suggests leadership principally evaluated on bonds of trust and identification (good trustees would display positive qualities of competence and rectitude). The delegate role, as an ideal type, suggests leadership principally evaluated on common policy actions and issue agendas.

While as normative ideals these types are drawn as distinct, we can assume that, empirically, elements of each may have some influence on leadership appraisal.[10] Of course, specific candidates are likely to display features relevant to each ideal type of representative and therefore elicit assessments by the public on standards derived from each type. If we can identify characteristic features of candidates that are apparently linked to leadership, such as trust and identification, and features of candidates that are apparently linked to policy, then it would be possible to test whether the former are likely to elicit feelings of confidence and enthusiasm and whether the latter are likely to elicit feelings of threat. We can hypothesize that personal characteristics of a candidate, features that suggest personal control and probity, are more likely linked to feelings of mastery and that issue positions, describing plans to shape the environment in particular ways, are more likely linked to feelings of threat.

Studies of candidate appraisal have found that a variety of considerations may play a role in assessing candidates (Kinder 1986; Miller, Wattenburg, and Oskana 1986). While these studies have found that respondents utilize a variety of considerations, the various considerations can be divided into two domains. The first domain contains appraisal of moral leadership (integrity, empathy, honesty, and the like). This domain identifies features of a candidate that are likely to elicit feelings of respect and trust. The second domain identifies appraisal of command: Does the performance of the candidate yield, whether from luck or skill, positive outcomes? This second domain contains appraisals of performance in meeting the crises and demands of the world at large. The failure of a leader to meet the confronting challenges successfully should, according to the task management–environment regarding distinction elicit more feelings on the threat dimension than on the mastery dimension of emotional appraisal. Though, if the failure is perceived to be due to a failure of character, depression rather than anxiety may be a dominant feeling among followers. The 1984 NES contains variables that can be used to construct measures of both moral leadership resources (the first domain) and leadership competence (the second domain).

The 1984 NES contains a number of different assessments of the two presidential candidates: their personal characteristics (15 measures) and Reagan's job approval ratings (4 measures). In addition, questions ascertaining issue positions were used to construct measures of the closeness of the respondent to the two candidates on leading foreign policy issues and domestic policy issues. For Reagan we can assess leadership competence by combining the measures of Reagan's job approval (a simple additive scale combining the four measures of Reagan's job approval, V258, V260, V262, and V264. Cronbach's $\alpha = .88$).[11] A comparable set of measures for Mondale is not available.

For both candidates the personal characteristics of the candidate can be assessed by using the 16 assessments for Reagan (V319–34) and Mondale (V335–50). Factor analyses of these 16 assessments ("How well does the word _____ describe _____?") shows a single factor accounting for approximately 60% of the common variance for both candidates (the second factor in each case accounts for less than 10% of the common variance). Other studies have found more dimensions (Miller, Wattenburg, and Oskana 1986). A factor analysis of the 1984 NES measures finds that single factors best describe Reagan's characteristics and Mondale's characteristics. Five terms loaded high on both first factors: *hard-working, decent, compassionate, moral,* and *kind.* The common content of this factor can best be described as moral leadership (i.e., matters of rectitude and leadership resources). Simple additive scales for Reagan and Mondale were created from these five items (Cronbach's α = .88 for Reagan and Cronbach's α = .85 for Mondale).

Issue appraisal measures were created using the simple absolute distances between respondent (V122) and candidates on the liberal-conservative scale (V126 for Reagan and V130 for Mondale) and on the four domestic policy issues on which the respondent's position is ascertained (V375, V382, V401 and V414: government spending, minority aid, economic status of women, and government guaranteed living–guaranteed job). The reliabilities for the respondent-Reagan proximity scale on domestic issues (Cronbach's α = .81) and respondent-Mondale proximity scale on domestic issues (Cronbach's α = .76) are acceptable. Similar proximity scales were created on three foreign policy issues (V395, V388, and V408: defense spending, involvement in Central America, and cooperation with Russia) and acceptable reliabilities were obtained; Cronbach's α = .75 for respondent-Reagan and Cronbach's α = .67 for respondent-Mondale.

It would seem plausible to argue that perception of a candidate taking a different policy position would most likely generate feelings of threat (since, if elected, that candidate would attempt to move the society in directions not favored by the respondent). Thus we can hypothesize that the closer the respondent perceives the candidate to his or her position, then the less threatening; *and* that this appraisal should have little impact on feelings of mastery. On the other hand, insofar as the relationship between political leader and supporter is one of identification (as almost all varieties of theories of political leadership attest), then perceptions of the candidate regarding the effective handling of the tasks at hand should be more strongly linked to the elicitation of positive emotionality than to the elicitation of negative emotionality.

In the two regression equations that follow, a number of candidate characteristics, policy appraisals, and one dimension of emotional response are entered as independent variables predicting to the other of the two dimensions of emotionality. The purpose of the regression equations is to identify which candidate features are particularly related to each dimension of emotionality. The regression equations should not be interpreted as claims of causality. The reciprocal influences of emotional and cognitive appraisal remain insufficiently understood to be able to specify with any confidence the causal interplay of the various interpretive processes. There is a lively debate in psychology on this subject and little consensus (Fiske and Pavelchak 1985; Lazarus 1982, 1984; Tassinary et al. 1984; Tomkins 1981; Tsal 1985; Zajonc 1980, 1982). There is little doubt that emotional processes influence memory (Bower 1981) and that emotional and cogitive processes are mutually influential, but precisely *how* is currently not well understood (but see Millar and Tesser 1986). The primary purpose of these equations, then, is to gain a better understanding of the two distinct dimensions of feelings.

Table 1 presents the relationship of candidate characteristics and policy appraisals to the scale of threat (a simple, summated, equally weighted scale of the four negative affect terms, *angry, afraid, disgusted,* and *uneasy;* Cronbach's α = .74 for Reagan and Cronbach's α = .75 for Mondale). Table 2 presents the relationship of candidate characteristics and policy appraisals to the scale of mastery (a simple, summated, equally weighted scale of the three positive affect terms, *hopeful, proud,* and *sympathetic;* Cronbach's α = .68 for Reagan and Cronbach's α = .68 for Mondale). In order to obtain a minimally acceptable scale, in terms of reliability, the term *sympathetic* is retained for use in the scales for Reagan and for Mondale.

The findings generally support the threat-mastery distinction as based on self-initiating and environment-regarding appraisals.[12] For Reagan and for Mondale moral leadership is strongly related to both threat and mastery but more strongly associated with feelings of mastery than it is with feelings of threat. For Reagan, leadership competence—his job approval—is more associated with threat than it is with mastery: the ratio of the β for leadership competence to the β for moral leadership is about two to one.[13]

Of the six possible issue appraisals, closeness of the respondent to Reagan and to Mondale on ideology and domestic and foreign policy issues, three have a substantial association with threat. The farther Reagan is perceived from the respondent on foreign policy issues, the more threatening he is to the respondent (a relationship stronger than the influence of party identification). For Mondale, the crucial appraisal is perceived closeness on domestic issues. The farther Mondale is perceived from the respondent on domestic issues, the more threatening. Given the tenor and rhetoric of the 1984 campaign, the respective vulnerabilities of the two candidates match closely these findings; Reagan vulnerable on foreign policy and Mondale vulnerable on domestic policy (Pomper et al. 1985). For Mondale only, perception of distance from the voter on ideology has the consequence of evoking threat. This confirms the conventional wisdom that Reagan was generally not perceived in ideological terms. Table 2 shows that only on domestic policy and only for Mondale is closeness between candidate and respondent related to positive emotional response.

Party identification plays a more influential role in determining Mondale's appraisal than it does Reagan's (though this is probably influenced by the lack of a job performance assessment for Mondale; party identification may "pick up" that variance). Interestingly, the small but negative values for party identification and feelings of mastery associated with Reagan suggest that the most partisan Republicans had somewhat more reserved feelings about Reagan than less partisan supporters (perhaps as a result of continued conservative complaint that the Reagan performance in office had not done enough for the conservative agenda). More importantly, these small β's for party identification show that emotional response to Reagan was broadly shared and not very different whatever the partisan identification of the respondent. By comparison, Mondale evokes different feelings among Democrats than among Republicans. This suggests that Mondale's appeal (or lack thereof) was, much more than Reagan's, limited to the pool of Democratic party identifiers.

The equation for Mondale is less powerful than that for Reagan (the multiple R-squared for Mondale is some 20 points lower). Most of this difference is apparently due to lack of information on Mondale's competence. The importance of job approval in defining the emotional

Table 1. Regression of Feelings of Threat on Candidate Evaluations

	Reagan			Mondale		
Candidate Evaluation	*b*	β	*t*	*b*	β	*t*
Candidate characteristics						
Leadership competence (Reagan job approval)	-.47	-.44	-12.7			
Moral leadership	-.45	-.21	-6.8	-.56	-.20	-7.7
Party identification (Republican)	-.06	-.08	-3.1	.13	.21	6.9
Policy appraisals						
Domestic issues (closeness)	-.03	-.02	- .8*	-.18	-.15	−4.9
Foreign issues (closeness)	-.13	-.13	-4.7	-.09	-.05	-1.6*
Ideological closeness	-.06	-.06	-2.2*	-.10	-.10	-3.2
Emotional response						
Mastery	.13	.13	4.9	-.04	-.05	-1.6*
R^2		.52			.29	
Adjusted R^2		.52			.28	
F		208.90			85.71	
df		7, 1369			6, 1273	

Source: 1984 NES.

*Not significant at $p = .01$.

Table 2. Regression of Feelings of Mastery on Candidate Evaluations

	Reagan			Mondale		
Candidate Evaluation	*b*	β	*t*	*b*	β	*t*
Candidate characteristics						
Leadership competence (Reagan job approval)	.39	.36	10.2			
Moral leadership	.74	.33	11.5	.85	.28	11.5
Party identification (Republican)	-.06	-.06	-3.1	-.22	-.31	-11.3
Policy appraisals						
Domestic issues (closeness)	.05	.04	1.4*	.13	.10	3.4
Foreign issues (closeness)	.05	.05	1.9*	.06	.05	1.8*
Ideological closeness	.06	.05	2.2*	.05	.04	1.5*
Emotional response						
Threat	.13	.13	4.9	-.05	-.04	-1.6*
R^2		.53			.34	
Adjusted R^2		.53			.34	
F		224.8			111.5	
df		7, 1369			6, 1273	

Source: 1984 NES.

*Not significant at $p = .01$.

characterization of President Reagan might, as many have concluded, suggest the benefit of incumbency to candidates for reelection. One handicap facing the challenger is that demonstrating ability to perform in the office must be made without the electorate having the opportunity to observe the challenger in the role of office holder (unless the challenger previously held the office). Of course, some incumbents find their history not so valued an asset (Ford and the pardon of Nixon; Carter and the perceived economic record), and in this instance Mondale has been Carter's vice-president. There is no formal evaluation of Mondale's expected job performance available in the 1984 NES data, so the two analyses are not precisely equivalent. Apart from this difference, the pattern of findings is remarkably similar.

On balance, the pattern of findings suggests that feelings of threat are evoked by perception of job performance, by perceptions of moral leadership, and by policy appraisals. The findings also suggest that feelings of mastery are evoked equally strongly by appraisals of moral leadership rectitude and leadership competence and more weakly by party identification and perhaps some policy appraisals. Finding that each dimension of affect covaries in different ways and to different degrees with policy appraisals and leadership characteristics is consistent with the theoretical assertion that each system of emotional response has a different strategic orientation.

Vote Disposition

Evaluating how much influence cognitive and emotional appraisals have, especially with respect to causality, is problematic, given the current state of knowledge. Brody and Page (1972) argue that distinguishing causality is difficult because issue positions, perceptions, and evaluations may be subject to persuasion and projection, thus making differentiating causal influences among the proximity judgments difficult. Further, a national cross-sectional survey is not a research design best adapted to the unraveling of the interplay between various sequential and simultaneous relationships. Nonetheless, we can begin to assess the relative influence of the various assessments considered on the propensity to vote for presidential candidate by examining the associations between the different influences and vote intention.

Brody and Page (1973), Kelley and Mirer (1974) and Rahn and her colleagues (n.d.) argue that people compare candidate images, summarizing judgments and feelings so as to form a voting decision based on which candidate fares better in the comparative process. Rahn and collegues go on to show that using comparative measures consistently produces a more successful predictive model than do models using the respondents' positions on the issues and the respondent's assessments of the candidates' characteristics.

The comparative perceptions approach is replicated here for most of the candidate characteristics. A measure for each respondent was created by subtracting Mondale's leadership scale score from Reagan's leadership scale score. The procedure yields a positive value when the comparison advantages Reagan, zero when the comparison is seen as tied and negative when the comparison advantages Mondale. The scores describe the extent to which a respondent perceives Reagan or Mondale to be the better leader, in the sense of more hardworking, decent, compassionate, moral, and kind. The more positive the scores, the more likely the respondent will decide to vote for Reagan; the more negative the scores, the more likely the respondent will decide to vote for Mondale (insofar as that comparison is influential). Similarly comparative judgments are measured on ideology

Table 3. Regression of Vote Disposition on Comparative Candidate and Policy Evaluations

Candidate Evaluation	*b*	*β*	*t*
Candidate characteristics			
Leadership competence (Reagan job approval)	.27	.30	10.7
Moral leadership[a]	.04	.03	1.2*
Party identification (Republican)	.09	.15	6.9
Policy appraisals			
Domestic issues[a]	.06	.08	3.3
Foreign issues[a]	.03	.04	1.9*
Ideological[a]	.00	.00	.2*
Emotional response			
Mastery[a]	.14	.27	10.3
Threat[a]	-.08	-.14	-5.7
R^2		.79	
Adjusted R^2		.79	
F		489.3	
df		8, 1016	

Source: 1984 NES.

[a]Comparative closeness.

*Not significant at $p = .01$.

(the respondent's distance from Mondale subtracted from the respondent's distance from Reagan), domestic policy issues, foreign policy issues, mastery (subtracting the mastery scale for Mondale from the mastery scale for Reagan for each respondent), and threat.

Leadership competence is available only for Reagan, as previously noted, so this is the only measure that is not comparative. I assume, with Rahn and her colleagues, that party identification incorporates a comparative judgment. Unlike Rahn and her colleagues, I treat affect as two-dimensional, with candidates being compared on the degree of feelings of mastery and on the degree of feelings of threat. Table 3, on vote disposition (using the four-point scale for vote disposition),[14] is regressed on candidate characteristics, party identification, policy appraisals, and the two factors of emotional appraisal, threat and mastery. As the vote disposition scale is coded so that intention to vote for Reagan is high, a positive value predicts to a greater intention to vote for Reagan and a negative value predicts to a greater intention to vote for Mondale.[15]

A positive assessment of Reagan's competence and the comparison of feelings of mastery elicited by the two candidates are the two most important factors in predicting a vote disposition favoring Reagan (*β*'s of .30 and .27 respectively). Next in importance are party identification ($\beta = .15$) and feelings of threat ($\beta = -.14$). Consistent with the LISREL analysis of the feeling measures and the feeling thermometers for Mondale and Reagan, positive feelings are more weighty than negative feelings (a ratio of 2 to 1).[16] As a group the policy appraisal measures do badly in predicting the vote. Only the comparison of perception of closeness on domestic issues reaches statistical significance although its influence ($\beta = .08$) is weak compared to the assessment of Reagan's competence, the two emotional appraisals and party identification. The overall strength of the model is quite high, the adjusted $R^2 = .79$.[17]

It is striking that issue appraisals have little apparent direct influence on voting. Feelings about the candidates, rather than thoughtful assessments regarding public policies, appear to be central to the voters' choices. But perhaps it is premature to conclude that issues have little direct influence in determining the outcome of elections. After all, few candidates publicly assert that issues do not matter. A frequent conclusion of voting studies is that the electorate does not engage in careful policy analysis and comparison of the party platforms offered by the major parties of candidates. This conclusion is frequently held to demonstrate that the electorate does not meet the standards required by "classical democratic theory." The obvious references are to Schumpeter, Berelson, and the Michigan school. Perhaps, as is frequently asserted, the public at large does not perform to expectations, but there is a competent political strata that does meet these standards.[18] If, so the argument goes, we divide the electorate into levels of competency, we can discern a more active and influential segment that does have the cognitive skill and experience to utilize the necessary sophisticated skills required of a democratic electorate.[19]

In the preliminary analyses that follow three cautionary points should be kept in mind. First, the combination of stratification of the sample and listwise deletion of missing values reduces the size of the subsamples, in some cases substantially. Second, the measurement of cognitive ability and its effects may be better attempted in experimental research designs than in secondary analysis of a cross-sectional national sample. And third, the 1984 NES data are collected quite late in the impression formation process and as a result may be less helpful in illuminating how impressions are formed and evaluating what role cognitive ability and emotional appraisal, respectively, play.

Various measures have been put forth to define and measure electorate competency, among them different indicators of cognitive complexity, ideological sophistication, and constraint (Neuman 1986). There still remains little consensus on how best to measure cognitive ability in politics (but see Lushkin 1987). We need not select such a measure, as we are here concerned with the search for segments of the electorate that eschew emotional responses to the candidates and rely instead upon the policy positions and ideological perspectives of the candidates. Perhaps the single measure on which most attention has centered is education. While education may not measure cognitive ability in politics directly and may contribute less to its development than expected, most democratic theories expect that education prepares people for citizenship (see, e.g., Thompson 1971). Further, educational attainment has often been used as one measure of sophistication, as it measures the ability and willingness to acquire cognitive skills as well as to learn citizenship (Neuman 1986). If lack of sophistication is the reason for so few using policy considerations to decide which candidate to support, then we can expect to find that stratifying the electorate on education will reveal a difference between those who rely upon feelings about the candidate and those who rely upon policy considerations.

Table 4 presents the same analysis as in Table 3, the regression of vote disposition on candidate characteristics and policy evaluations, but with the data stratified into those with less than a high school diploma, those with at least a high school diploma, and those with some college and postsecondary education. Only unstandardized regression coefficients are provided (*b*s), as the relevant comparisons are between education strata.

The only suggestion that education divides the adult population into strata, each with its own mode of reasoning, is that assessment of Reagan's leadership

Table 4. Regression of Vote Disposition on Comparative Candidate and Policy Evaluations: Stratified by Education

Candidate Evaluation	Low Education *b*	Low Education *t*	Medium Education *b*	Medium Education *t*	High Education *b*	High Education *t*
Candidate characteristics						
Leadership competence (Reagan job approval)	.12	1.8*	.38	7.7	.28	8.5
Moral leadership[a]	.23	2.0*	.03	.4*	.01	.3*
Party identification (Republican)	.22	5.8	.05	2.0*	.08	4.6
Policy appraisals						
Domestic issues[a]	.06	1.2*	.04	1.5*	.07	3.1
Foreign issues[a]	.25	1.2*	.13	1.1*	.16	1.6*
Ideological[a]	.02	.6*	.00	.0*	.03	1.6*
Emotional response						
Mastery[a]	.13	3.0	.14	5.9	.12	7.1
Threat[a]	-.06	-1.3*	-.09	-3.3*	-.08	-5.1
R^2	.83		.80		.80	
Adjusted R^2	.82		.78		.79	
F	60.5		145.2		264.9	
df	8, 100		8, 310		8, 588	

Source: 1984 NES.

[a] Comparative closeness.

*Not significant at $p = .01$.

competence and moral leadership has little influence on vote disposition among the less educated; in this strata party identification has a substantial association with vote disposition. This might suggest that the less educated, compared to the more educated, rely more on partisan cues and less on candidate characteristics. Studies by Lau (1986) and Miller, Wattenburg, and Oskana (1986) find that the more educated have more political information than the less educated.

Only the domestic issue closeness comparison is significantly related to the vote disposition in the more educated strata (which suggests that issues may have some influence on voters actions, but most likely this influence is an indirect effect through emotional response). But apart from these differences, the perception that one candidate is ideologically closer to the voter, is closer to the respondent on foreign policy issues, or closer to the respondent on domestic issues does not seem to play a greater role in the vote decision for the more educated than for the less. Moreover, comparative feelings about the candidates seem, if anything, more influential among the more than the less educated (and the ratio of the influence of feelings of mastery to the influence of feelings of threat remains rather constant at two to one, suggesting that across the different educational strata, feelings of threat have about half the impact of feelings of mastery on vote decisions).

Stratifying the sample by interest in politics, another factor commonly held to lead to political sophistication (Neuman 1986), finds even less evidence of greater reliance on cognitive cues among the higher strata. All of the nine policy appraisals, three within each stratum, have weak coefficients and none are statistically significant (Marcus 1987). The coefficients for the two dimensions of emotional response are weaker in the least interested stratum (.10 and −.06 for mastery and threat) and stronger in the moderate and high interest strata and display the same two-to-one ratio found in the analyses above (.17 and .15 for mastery and −.07 and −.09 for threat). These results suggest the not-surprising relationship that interest in politics and passionate engagement toward or against candidates is linked.

One explanation for why issues play so little influence in candidate choice is offered by Page (1976, 1978). Page's analysis of party platforms and candidate positions concludes that candidates avoid controversial issues and attempt to present ambiguous positions that can appeal to the broadest range of likely voters. Obtaining definitive, or even moderately accurate, information on the positions of candidates on even the most controversial issues of the day may be quite difficult and therefore prevent many voters from issue-voting even if they are so inclined (Page and Brody 1972). Yet for those issues that are important to the voter, issue comparisons may play an important role. To evaluate this possibility, the sample was stratified into thirds by the degree of importance of three domestic issues on which the 1984 NES ascertained how important the issue is to respondents (government spending, economic status of women, and government guaranteed living-guaranteed job; V380, V407, and V420 respectively).[20] The results essentially mirror those in Table 4. None of the *b*s for the nine policy appraisals, three in each stratum of importance, has a significant influence on vote disposition. As in the previous tables, the influence of the two dimensions of feeling is stable across the three strata (mastery has *b*s of .15, .13, and .15 and threat has *b*s of −.05, −.08, and −.07 across the three strata).

The results are noteworthy for the consistent reliance on emotional response in the sample as a whole and within various subsamples. While these findings must await confirmation from more appropriate research designs before being treated as more than suggestive, the basic finding of pervasive and uniform reliance on emotional response is consistent with a number of studies in politics (Kinder and Sears 1981; Kuklinski et al. 1987; Sears et al. 1980).

Notwithstanding these findings, it would be incorrect to conclude that issues do not play an influential role in voters choices between presidential candidates. Table 1 presents results that suggest that issues and policy considerations do influence voters by acting upon emotions and particularly on the environment-regarding dimension of emotion, feelings of threat. Emotions are rarely, in the Western tradition, taken as rational or of adaptive benefit, although David Hume is one notable exception (Lloyd 1984). But emotions may provide people with an efficient means of comprehending their circumstances. Reliance on emotional response to characterize candidates on leadership ability, competence, and issues and policy positions provides the electorate with a speedy and efficient means of determining candidate suitability on criteria that are consistent with the requirements of republican forms of government.

Conclusion

The circumplex model provides an economical approach to the study of emotional response. More importantly, by demonstrating that emotionality consists of two concurrent appraisal processes, one strategically dedicated to self-initiating action and one strategically dedicated to evaluating threat, the results reported here resolve the anomalous findings of previous studies that positive and negative emotions are often concurrently reported.

The relationship between the structure of emotions and candidate appraisal can also be useful in gaining a better understanding of the likely success of different strategies of political campaigning. The consistent finding that positive emotional response to the candidates is more influential than negative emotional response suggests that elections turn more on moral leadership and leadership competence and less on issues. Issue appraisals do not seem to have much, if any, influence on the emotional enthusiasm created by candidates. Yet issues are shown to have an influence on the threat dimension of emotional response. This suggests that running a campaign based on matching the issue positions of the voters has limited prospects for strengthening a candidate's image of leadership and competence. Indeed, as Gary Hart's decline in the summer of 1987 suggested, running a campaign solely "on the issues" may create a disquieting sense that something is being hidden or that there is something lacking in the character of the candidate. This in turn is likely to provoke press inquiries into the character of the candidate.

Insofar as issue positions of a candidate are seen to be discrepant from the issue positions of the voters, candidates risk arousing feelings of anxiety and threat among voters. Thus candidates may best use issues to forestall others from using issues against them, as in negative campaigning, or from taking positions through miscalculation that place them in opposition to their supporters.

It is important to add that threat feelings are elicited by the prospect of an unpredictable environment as well as by that of a hostile environment (Gray 1981). This would suggest why candidates are so quick to paint their opponents as "flip-flopping on the issues." Successful campaign rhetoric of this genre can make the opposition candidate vulnerable by depicting the candidate as uncertain and likely to take unpredictable positions. It would seem that the most successful campaign strategy would try to build support through depicting a candidate as competent to produce good results in the job and of good probity and to use issues only defensively (to sustain calm feelings rather than build enthusiastic support).[21] Position taking, if misguided, can hurt. But unless necessary to respond to challenges raised by the opposition, position taking is not likely greatly to help a candidate build enthusiasm.

Finally, these results suggest, as has been long noted, that public debate is often accompanied by heat and passion. Madison identified an important relationship between emotions, deliberation, and the attention given to public measures: "It is a measure of misfortune, inseparable from human affairs, that public measures are rarely investigated with that spirit of moderation which is essential to a just estimate of their real tendency to advance or obstruct the public good; and that *this spirit is more apt to be diminished than prompted by those occasions which require an unusual exercise of it*" (*Federalist Papers*, no. 37; italics added). Madison's insight is that passion (or to use our terminology, *emotional response*) is what draws individuals to politics. Madison is properly skeptical of the influence of emotions, but he recognizes that emotions are an unavoidable and dynamic part of politics.[22] Emotions do not just form empty feelings, are not merely subjective; rather, they constitute vital strategic understandings (Marcus and Rahn n.d.). While the process by which these emotional responses are arrived at is hidden (Tsal 1985) and is likely to be fallible, emotionality nonetheless does serve instrumental purposes. It is perhaps appropriate to reexamine the received wisdom that emotions constitute "a regrettable imperfection in an otherwise perfect cognitive machine" (Scherer 1982).

Table A-1. Regression of Vote Disposition on Comparative Candidate and Policy Evaluations with Feeling Thermometers To Assess Affect

Candidate Evaluation	*b*	*β*	*t*
Candidate characteristics			
Leadership competence (Reagan job approval)	.27	.30	10.6
Moral leadership[a]	.01	.01	.4*
Party identification (Republican)	.09	.15	6.8
Policy appraisals			
Domestic issues[a]	.05	.07	3.3
Foreign issues[a]	.02	.04	1.8*
Ideological[a]	−.01	−.02	−.9*
Emotional response			
Feeling thermometers[a]	.01	.42	12.0
R^2		.79	
Adjusted R^2		.79	
F		555.3	
df		7, 1026	

Source: 1984 NES.

[a]Comparative closeness.

*Not significant at $p = .01$.

Appendix: The Measurement of Emotional Response

There is a further test available to assess the claim that the feeling thermometers are confounded measures. The regression presented in Table 3 can be reestimated with the difference between Reagan and Mondale feeling thermometers replacing the two scales of mastery and threat. If relationships are different when the feeling thermometers are used, then it would suggest that the feeling thermometers covary with these independent variables in ways not captured by the two scales of mastery and threat. Table A-1 presents this regression analysis.

The results, except for the measurement of emotional response, are almost identical to those reported in Table 3, with no differences greater than .02 for standardized or unstandardized regression coefficients, *R*-squared or adjusted *R*-squared values. This might suggest that the two-dimensional model adds little to what can be gained by using the feeling thermometers. However, this conclusion would be a substantial mistake. The feeling thermometers mask two quite different appraisal processes.

The thermometer scale, as a measure of emotional response, is a confounded measurement device. Substantively, responses to feeling thermometers will be an indistinguishable mixture of the intensity of threat and the intensity of mastery evoked by the object of appraisal. Continued reliance on thermometer scales to assess global feelings regarding an object of appraisal would be ill advised.

The feelings measures developed for the National Election Studies are an improvement over the feeling thermometers. They provide multiple measures of the two dimensions and these indicators enable the two dimensions to be discriminated from each other. However, they suffer three liabilities. First, one of the positive mood terms, *sympathetic*, is not a good measure of mastery. It is less reliable than is generally desired. And its location in the circumplex, Figures 1 and 2, and the two LISREL models, suggest that sympathy is a compound mood term and should be dropped as a measure of the mastery dimension. Adding one or two additional mastery emotion indicators would seem advisable (see Watson and Tellegen 1985, for numerous alternatives). Second, the feeling measures are dichotomous, thus sacrificing the range of responses offered by the feeling thermometers. Third, current emotional response measures do not identify the nonaffective pole of each dimension. An alternative procedure would be to use semantic differential formats with the appropriate mood term anchoring the low-arousal pole scale and the appropriate mood term anchoring the high-arousal pole of the scale. For example, mastery mood terms and their low-arousal polar opposites such as the following could be used: *indifferent, curious, enthusiastic, unenthusiastic; interested, withdrawn*; and *depressed, happy*. For threat mood terms and their low-arousal polar opposites the following could be used: *tense, calm; upset, comfortable; worried, contented;* and *nervous, relaxed* (Marcus 1985). Using this measurement approach each measure would provide a wider range of scores than the current measures. Further, using this proposed measurement approach would provide for multiple measures and thus ensure ample opportunity for item analysis (reliability and validity assessment) and enable scale building and multiple indicator methods of analysis.

Notes

The data utilized in this analysis were made available by the Inter-University Consortium for Political and Social Research. The data for the 1984 American National Election Study were originally collected by the University of Michigan, Center for Political Studies of the Institute for Social Research for the National Election Studies under the overall direction of Warren E. Miller. Santa Traugott was director of studies in 1984. The data were collected under a grant from the National Science Foundation. I thank Kimberly Jordan for help in conducting the statistical analyses, and Andy Crider, George Goethals, Saul Kassin, Donald T. Campbell, W. Lance Bennett, Tim Cook, Roger Masters, Lyn Ragsdale, Wendy Rahn, and John L. Sullivan for their useful comments and suggestions.

1. In the data used here, the correlations between feeling thermometers for Mondale and Reagan and vote intention (V425) in the 1984 NES are .50 and .53 for Mondale and Reagan respectively. Further, if we use the strength of voting preference (V426) and voting intention to create a four-point scale, the correlation between Reagan's feeling thermometer score and the vote intention scale increases to .78, and the correlation between Mondale's feeling thermometer and the vote intention scale increases to .71 (the correlation between the vote intention scale and V425 is .94; the increases in correlation between feeling thermometers and vote intention scale result from using a scale with more categories [see Bollen and Barb 1981]).

2. It is necessary to differentiate between conscious and nonconscious cognitive processes, each of which can influence behavior, as well as between the interdependent influences of emotional and cognitive processes (Lewicki 1986; Millar and Tesser 1986; Tassinary et al. 1984).

3. For Mondale, the first factor has an eigenvalue of 2.62 and the second factor has an eigenvalue of 1.54; together the two factors account for 60% of the variance. For Reagan, the first factor has an eigenvalue of 2.74 and the second factor has an eigenvalue of 1.37; together the two factors account for 59% of the variance. In each case the third eigenvalue is well below 1.00 (.77 and .78 respectively).

4. Fonberg (1986) reviews the neurophysiological basis for the two-system paradigm.

5. While the mastery dimension is hypothesized to be self-orientated, the capacity for identification with others (e.g., empathy and group identification) extend appraisals of mastery to collective action and to the actions of others.

6. The full Gray-Tellegen model accounts for mood and temperament (stable emotional differences). Temperament can be defined as reactivity levels. Temperament is the disposition, across situational states, to interpret any environment as, for example, threatening. Because the NES data, used here, does not contain any acceptable measures of temperament, this aspect of the Gray-Tellegen model can not be pursued.

7. The correlation matrices are positive definite. Reestimating with maximum likelihood gives essentially the same solutions. Also, defining the model with three latent concepts, threat, mastery, and feeling thermometer—the latter defined by one indicator—provides the same solution, differing only in that rather than epistemic correlations running from

mastery and threat to the feeling thermometer indicator, the solution has structural parameters running from mastery and threat to the latent concept, feeling thermometer. The values are the same.

8. It may also be the case that there is a third dimension of emotion, *empathy*, which has been shown to have a cognitive and affective dimensions (Davis 1983; Chlopan et al. 1985). The collection of feeling measures in the NES series does not enable this possibility to be pursued. In any case, this and other studies (Masters and Sullivan 1986) suggest that the two dimensions of circumplex model account for the predominant variance in feelings. See n. 3.

9. See n. 4.

10. Sullivan and Masters (1988) have shown using experimental procedures that emotional displays by presidential candidates evoke two dimensions of emotional appraisal in observers, *hedonic* and *agonic*, which are equivalent to the two dimensions conceptualized here as mastery and threat.

11. Scale scores reported for these and all subsequent scales are the average values of the sum of all valid responses when the number of valid responses is equal to, or one less than, the maximum number of items in the scale. For example, a scale with four items will return a scale score if respondents provide valid responses to any three, or all four, scale items. This preserves more cases for subsequent multivariate analysis (which, using listwise deletion, can sharply reduce the number of cases available for analysis).

12. With samples as large as those in the NES series, statistical significance is readily achieved for all but the smallest relationships. Substantive assessments must be made, rather than having interpretation rest on the issue of inferential risk.

13. Since I am interested, in this analysis, in comparing the relative influence of different independent variables and as the scales are "scale-free" (that is, the range of the scale scores is a result of the available number of items making up each scale), the relevant statistic is the β, the standardized regression coefficient.

14. See n. 1.

15. It has been suggested that because competence as a measure of leadership is available only for Reagan, this measure should be removed from the comparative model, in this and in other analyses to follow. Of course, as the incumbent, only Reagan can be evaluated by the voters on presidential leadership performance. For Mondale, voters may extrapolate from Mondale's campaign activity as reported and seen, current policy positions, and from his past record as a political leader. Many voting theories do assume comparison between an incumbent's record of performance and the challenger's positions. Theoretical issues aside, removing Reagan's job approval from the analysis does not change the results in any principal way. All coefficients increase marginally by about the same small amounts. The R-squared drops by an average of .05. More importantly, policy appraisals remain uniformly small and emotional responses become more influential. Alternative tables, with leadership competence removed, are available from the author.

16. The proportion may well change from candidate to candidate and from campaign to campaign, depending on the particular strengths and liabilities of specific candidates. More studies will be needed to ascertain how labile these appraisals are and precisely what kinds of information have most consequence for emotive appraisal.

17. Consistent with the findings reported in Rahn and colleagues n.d., the comparative model does better than models that predict vote choice based on ideological and issues closeness and level of threat and mastery reported for each candidate (i.e., a noncomparative model). For the model predicting vote for Reagan, using distance measures between Reagan and the respondent, the adjusted R-squared is .75. For Mondale, the adjusted R-squared is .76 when using Mondale's characteristics and closeness to the respondent (including Reagan's job performance in the Mondale model).

18. This tradition has flourished in areas other than voting, for example in the analysis of who comprehends and subscribes to the rules of the game and political tolerance (Mueller n.d.).

19. The studies that find such strata have been countered by studies that dispute the methodology of the studies and the findings themselves (Sullivan, Piereson, and Marcus 1982). But on balance the more accepted position seems to be that put forth by Lippmann, Schumpeter, and others in that tradition (for a review, see Krouse and Marcus 1984).

20. In order to identify the strata, those who identified an issue as *extremely important* were assigned a score of three on that issue; those who identified the issue as *very important* were assigned a score of two on that issue; those who identified the issue as *somewhat important* were assigned a score of one on that issue; and all others were assigned a score of zero. The scores were aggregated to form an index. All those with a total of three or less are the low-importance strata; those with a total of four or five are the medium-importance strata; and those with a total greater than five are the high importance strata.

21. Selective examples show what can transpire when political campaigns fail to sustain the candidate image as strong and competent; for example, Gary Hart, Edmund Muskie, and George Romney quickly come to mind as candidates who failed in precisely this fashion. Personal stability and strength would appear to be minimal requisites for competence.

22. The principal focus of Madison's skeptical attention, in *Federalist Papers*, no. 37, is on the fallibility and partiality of reason; a common con-

cern in contemporary social science (Greenwald 1980; Marcus and Rahn n.d.; Markus 1986; Nisbett and Ross 1980).

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AGE AND ACTIVE-PASSIVE LEADERSHIP STYLE

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The relationship between age and active-passive leadership style is analyzed with direct observational data describing the behavior of a sample of 24 mayors during issue discussion and debate on their councils over a one-year period of meetings. Activity, measured as a time-based rate of verbal participation, was found to increase with the age of leaders to a high in the midfifties, and to decline for older mayors in their sixties and seventies. Political experience interacted with age to affect activity. Inexperienced mayors displayed a less active style, regardless of age; but experienced mayors in their sixties and seventies had the least active style of all. These findings held up under control for group size, political structure on the councils, and community characteristics.

The Iran-Contra travails of the Reagan presidency raise and illustrate two potentially interrelated problems of leadership. One problem involves the limitations of passive leadership, and the second, the effects of old age upon role performance. Reagan's passive approach to leadership is legend and this style is offered in partial explanation of his failure to monitor and control the activities of his advisors and subordinates more thoroughly. The question was first posed (in the media) with the first Reagan-Mondale debate in 1984, whether a man (at least, this man) in his mid-seventies was physically capable of fully exercising the responsibilities of presidential leadership. Barber (1977) describes the dimensions of "presidential character"—active-passive, positive-negative—as fundamentally fixed early in the political experience of a leader. The interesting question raised by the Reagan case involves the possibility that the active-passive dimension of leadership, regardless of any initial disposition associated with personality and character, may vary systematically with older age toward less activity in role performance.

Aging and Leadership Style

This argument does not propose that barring serious illness, the physiological effects of aging beyond, say, 65 will turn an active leader into a passive leader; and no doubt there are many active elderly leaders. Rather, the more realistic proposition is that an active leader will become less active, and a passive leader, more passive. A model posing a relationship between age and leadership activity is not alternative, but complementary to Barber's emphasis on personality and character in the prediction of leadership performance.

Active-Passive Leadership

The active-passive dimension of leadership is defined by Barber (1977, 11) in terms of energy investment in job performance. He asks, "How much energy does the man invest in his Presidency?" The concept of activity is not defined abstractly but in concrete physiological terms. Activity involving energy expenditure may be indicated by the frequency of acts of role performance in the task areas that comprise leadership behavior. Although leaders may emphasize different areas of task performance, an underlying question remains of how much effort they put into their work. For any leader, an "activity" dimension of style involves energy investment in job performance.

There is no presumption in this definition of active-passive leadership of any inherent value associated with either active or passive extremes. It is not clear, a priori, that passive leaders are somehow poorer or less effective than "active" leaders in their roles. The adaptive value of any particular style should vary systematically with properties of the broader political context. Indeed, it is vital to distinguish between aspects of character and style in role performance and evaluations of outcomes or effectiveness because these are, respectively, independent and dependent variables. It is certainly plausible that a tendency toward less active leadership associated with older age may be compensated by more effective, skillful, and thus efficient leadership for some individuals.

Age

Chronological age is a marker variable indicating in quite relativistic terms the stage of the biological aging process and, less accurately, the social-life stage as well. It is biological aging that may most clearly bear upon leadership style and it should be kept in mind that aging processes occur at differential rates across individuals. Chronological age provides only a crude indicator, not a direct measure, of the process of biological aging; therefore a portion of error is built into time-based observations.

Fundamental to the hypothesis is the changing ability of individuals to expend energy in job performance, particularly the bearing of age on arousal, attention span, vocal production and sound processing, sight and visual processing, memory, reaction time, learning rate, physical strength, adaptability to stress, and susceptibility to disease (Birren and Schaie 1985; Botwinick 1978; Fries and Crapo 1981). As Comfort (1979, 16–17) observes, "Vigor declines and disease susceptibility multiplies with increasing age" and "the rate of this increase under the best conditions has a characteristic value in each species and is exponential." He notes that the rate of decline in vigor increases after about 40 in humans, "the end of the plateau of adult vigor" (p. 300). Certainly, biological aging effects should become increasingly pronounced in elite behavior by late middle age (Schubert 1983). One net effect of biological aging in older age must be to constrain physically demanding activity.

Research Design

To explore support for the proposition that aging is associated with less active leadership style, data were utilized from a broader study concerned with age and political decision making in small groups (Schubert, Wiegele, and Hines 1986, 1987). In this study, meetings of 24 municipal councils were observed over a one-year period. Proceedings of meetings were recorded on audio tape and written transcripts were prepared for periods of active discussion and debate. The 24 councils included samples of eight, drawn from three regions of the United States: East, Midwest, and South. These councils governed typically smaller rural and suburban communities, with total populations ranging from 95 to 70,000 in the

1980 census.[1] Their mode of operation bore little resemblance to the televised melodramatics of big city councils in metropolitan areas and their procedural process and discussion were characteristically informal and relaxed, with substantial spontaneity.

The audio and printed transcript materials provided the basis for coding behavioral events. Specifically, observations were recorded for each speech segment or "speaking turn"—the period an individual holds the floor as speaker—that occurred during council discussions. Approximately 40 thousand turns were coded for the year of meetings across the 24 councils. For this analysis, data were aggregated to the level of the individual over the entire period of observation.

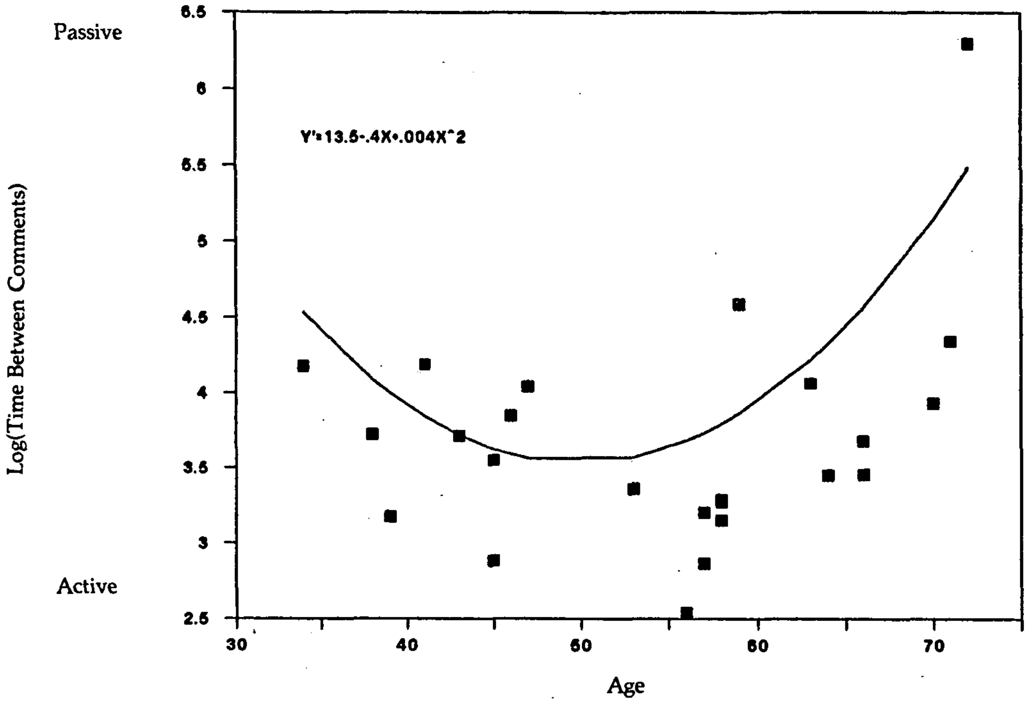
Data on chronological age were collected through personal interviews or, in a few cases where interviews were not granted, through a survey of city clerks. The 24 mayors ranged in age from the midthirties to early seventies about a mean of 54 years. Three of the mayors were in their early seventies, three in their midsixties, and six in their late fifties or early sixties. While these data clearly contain fewer data points for the ages in question than might be desired, they do provide a useful starting point for exploration of the research question because they bracket an age range that extends into "older" age and permit analysis and comparison across the actual age spectrum of political elites.

The general concept of leadership activity, defined as energy investment in job performance, was operationalized in terms of verbal activity in the management of group discussion. Because purely procedural comments (e.g., "Is there a second?") were not included in the data, scores describe only discretionary participation. Verbal activity was measured by the mayors' "commitment rate"—that is, total time (in seconds) used by all elected group members speaking during discussion, divided by the total frequency of mayor speaking turns. The resulting scores reveal, on the average, how often mayors spoke during group discussion. The scores range from a low of about once every 20 seconds, indicating high verbal activity, to highs exceeding once a minute, indicating low verbal activity. The comment rate was employed because it provides what is literally a behavioral activity indicator. Measures of relative activity, such as proportion of group comments or total discussion time, would not be appropriate indicators of the dependent variable because they do not describe how much activity occurred in terms that would be comparable across groups.

To guard against the possibility that some mayors might appear to have less activity because they made longer comments, mean length of turn was analyzed as a control variable and found to have no significant effects upon the findings. With respect to reliability, it should be noted that observers recorded the identity of speakers on a pocket computer, when they began speaking during the meetings, and these data were subsequently verified when coders listened to the audio tape in the final coding process. Speaking turns were also timed from the audio tape by computer.

Although managing meetings is but a small part of the leadership tasks of those in leadership roles, it is nonetheless a common behavioral setting across different levels and types of leadership and, in committee decision making contexts, it is an important task. Whether or not verbal activity in leadership during meetings bears upon leadership style in other task areas of leader performance is an empirical question that awaits systematic study. Without belaboring the obvious, actively chairing the discussion process in a small decision making group over the course of a two-to-four-hour meeting entails substantial energy expenditure. Active partic-

Figure 1. Age and Verbal Activity



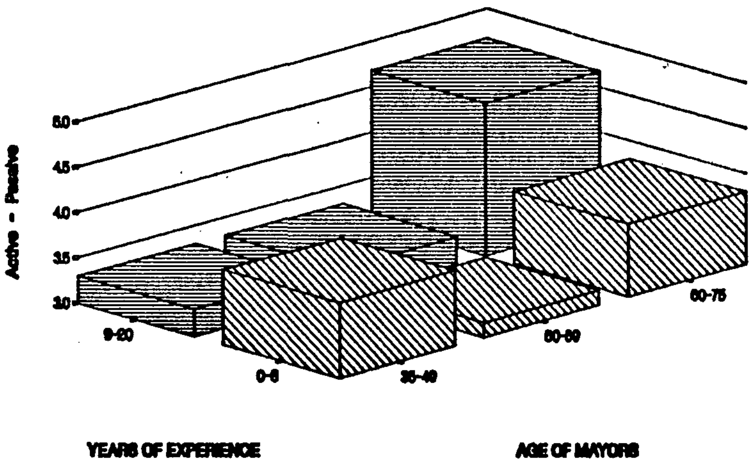
Note: $R^2 = .4179$; $F = 7.6$, $p < .003$.

ipation requires apt attention to virtually all aspects of proceedings. On small councils in local government, mayors often perform both task and socioemotional leadership roles and energy is expended both in attentive listening and in speaking. It is reasonable to assume that a leader who speaks on the average once every 20 seconds is investing a good deal of energy in job performance.

Findings

Because the comment rate scores revealed a pronounced positive skew, including one extremely inactive mayor, these data were transformed to natural logs. Figure 1 presents the scatter diagram for the relationship of age and logged comment rate. The simple bivariate correlations were .34 for the raw rate and .27 for the logged rate, describing a pattern of less verbal activity, the older the mayor. The plot also reveals a pronounced curvilinearity in the relationship. To model this effect a second-order polynomial function was fit to the data, using a nonlinear least squares estimation procedure, and it accounted for a statistically significant 42% of the variance in verbal activity. The pattern in these data is clear. Verbal activity is initially moderate and increases with age for mayors in their thirties and early fourties. It is high and stable for mayors in their midfourties through midfifties, but declines directly with greater age for those in their late fifties on. The pattern for younger mayors, which is less pronounced, is not predictable from the theoretical model. It is plausible that the

Figure 2. Age, Experience and Logged Comment Rate



Note: The "50s, 0-8" category has only one case.

initial increase in activity associated with older age for younger mayors reflects the increasing social status, self-assurance, and political experience associated with maturation into middle age. These factors should not diminish in importance with increasingly older age; yet a positive slope is quite clear from 55 on. Before accepting support for the research hypothesis, it is necessary to consider alternative explanations of the decline in activity associated with older age.

One possibility is that older mayors were less active verbally because they were more experienced and therefore more skillful in managing meetings. It is quite plausible that the loss of vitality in older age is offset or accommodated to some extent by more efficient use of energy in task performance. In a study of state legislators, Fengler (1980) found that, while attendance and legislative activity declined with age, success (rate of passed to initiated bills) increased with age. Thus older mayors might speak less often but with greater effect upon the group process. As with the effect of biological aging, this "experience" effect would be revealed in declining activity, regardless of any initial, baseline tendency toward "active" or "passive" leadership.

To explore this experience hypothesis, data (acquired during the personal interviews) were developed on each mayor's number of years of prior service on the council, either as mayor or as elected member. Among these 24 mayors, the correlation of age and experience was quite weak at .19 and the scatter diagram revealed no pattern. For 7 mayors over 60, 2 had less than 3 years' experience, 3

Table 1. Predicting Active-Passive Leadership

Equation	Predictor Variable	Regression Coefficient	Standard Error	Beta Coefficient	Two-Tail Probability
$R^2 = .42$; SEE $= .598$	constant	13.480	3.043	.00	.000
$F = 7.595, p < .003$	age	−.399	.117	−5.96	.003
	age (squared)	.004	.001	6.24	.002
$R^2 = .629$; SEE $= .516$	constant	14.360	2.933	.00	.000
$F = 7.624, p < .001$	age	−.391	.115	−5.55	.003
	age (squared)	.003	.001	5.24	.006
	experience	−.320	.124	−2.55	.019
	age * experience	.006	.002	2.91	.011
$R^2 = .654$; SEE $= .565$	constant	12.792	3.700	.00	.004
$F = 3.31, p < .024$	age	−.342	.136	−4.84	.025
	age (squared)	.003	.001	4.54	.037
	experience	−.334	.141	−2.67	.033
	age * experience	.006	.002	3.00	.021
	population	−.000	.000	−.02	.920
	% rural	.001	.003	.09	.689
	size	.059	.125	.10	.642
	political structure	−.116	.200	−.11	.572
$R^2 = .186$; SEE $= .744$	constant	3.030	.854	.00	.002
$F = 1.447, p < .261$	age	.021	.015	.30	.171
	experience	.004	.027	.03	.894
	political structure	−.360	.221	−.34	.120

Note: The dependent variable is the natural logarithm of the time in seconds between comments: lower scores describe a more active style.

had 5 to 10 years', and 2 had 10 to 16. Moreover, experience and the logged comment rate were virtually independent ($r = .03$), and this did not change when experience was added into the least squares equation with the age variables. However, investigation of a possible interaction effect of age and experience on activity yielded intriguing results. Figure 2 illustrates the two-way interaction by plotting the mean comment rate by experience, cut at the median, and age group. Although older mayors were less active overall than others, experienced older mayors were least active of all. In addition, the passivity of younger mayors noted above was associated with inexperience. Experienced younger mayors were as active as middle-aged mayors, and in both of these groups experience was manifest in an energy-intensive, not energy-conserving, leadership style.

Regression analysis with multiplicative terms was employed to estimate the influence of the interaction of age and experience on behavior. The results presented in Table 1 reveal that with the interaction term in the equation, the variance predicted improved from 46% to 63%, and all terms had significant effects. The polynomial effects of chronological age were most powerful, but experience and its interaction with age made notable contributions to the prediction. It should be noted that the partial effects for experience alone indicate more, not less, activity with greater experience. It is the interaction term that predicts to passive style. The standard errors for predictor coefficients are acceptable and the residual plots revealed no abnormalities. There was one apparent outlier (see Figure 1)—an elderly mayor with substantial experience who rarely spoke during his council's discussions. With his activity score truncated to 4.5, the regression was rerun with the effect of marginally decreasing the variance explained and the extent to which coefficients passed levels of significance.

The effects of three other intervening variables were considered in the analysis. Partisan *political structure* within groups was considered based on the hypothesis that management of groups with competing party blocs would require greater verbal activity by mayors. Ordinal measurement distinctions were introduced for (1) nonpartisan and one-party councils, (2) councils with 80% or more members from one party, and (3) those with weaker party majorities. *Size of group,* which ranged from five through nine including the mayor, was also considered because group size affects the opportunities for participation, albeit more for members than mayors. Finally, to explore the possibility that leadership style would be influenced by the complexity of issues and the informality of group processes occurring differentially in smaller rural and larger suburban community settings, 1980 census date on *total* and *percentage rural population* were considered.

Results from a multiple regression analysis that included age, experience, and the other variables are reported in Table 1. None of the added variables make even a modest contribution to the prediction of mayoral activity. However, it is interesting to observe from the final equation in Table 1 that without the multiplicative terms, partisan division is moderately associated with more active style and to note that total population is correlated with division in political structure (r = .44). The fact that these environmental factors washed out in the more comprehensive model indicates a causal structure in which environmental factors may bear upon the characteristics of office holders, but individual factors explain leadership style.

It may be clear from Figure 2 that the reason why the interaction term is so important is that inexperienced younger (mean = 3.83, s = .4, n = 6) and older (mean = 3.8, s = .43, n = 4) mayors were similarly passive and the real decline in activity with older age occured among those with substantial experience. Thus inexperience is associated with less activity regardless of age. Among older mayors, why were those with experience even less active? Of all the variables, the only major difference between these two categories was years of experience—13.7 to 4 years on the average. Among experienced mayors, why is the passive style of older mayors so different from the active style in the younger categories? Mean years of experience varies from 12 to 14.7 to 13.7 from young to old, and differences did not emerge on the other variables either. What separates these categories is a decade of aging—a mean of 56.9 years in the middle group compared to 67 years in the older group. It is plausible that the answers lie in the nature of experience and the nature of aging in older age. The reason why experience may differentiate behavior is that it allows flexibility in leadership strategies. The reason why age differentiates behavior among experienced leaders is that it affects choices among strategies available to leaders.

Discussion

The frequent citations of Barber's active-passive, positive-negative typology of leaders in both its presidential (1977) and legislative (1965) applications, reveal at least its heuristic value in describing important qualities of leadership. Present concern is limited to the active-passive dimension because there is no apparent theoretical connection between age and the positive-negative dimension. Despite the heuristic value of the active-passive distinction, questions of its theoretical significance may reasonably be raised.

There is a tendency to confuse active-passive style with effectiveness in role performance, with success in policy making, and with evaluations of "strong" and "weak" leadership. Although activity is conceptually independent of such dependent variables, its broader theoretical significance may ultimately be demonstrated by research into its effects upon such outcome variables. Just prior to the outbreak of information on the administration's Iran-Contra policies, for instance, a major national business magazine ran a cover story posing President Reagan's nominally passive leadership style as a model for the corporate United States. Up to this point, a low-energy leadership style—that was revealed in avoidance of detail, a relaxed approach to informational homework, even sporadic attentiveness and sporadic verbal participation in small group meetings—was widely (or plausibly) understood as positively adaptive in the president's role performance. The Reagan case clearly isolates and distinguishes the activity dimension of leadership from outcome and effectiveness questions and surely begs investigation of the theoretical relationship between style and outcomes.

Although it may be clear that activity in presidential leadership could have important political implications, the question may still be raised whether leaders' activity in managing committee-style decision making groups bears on significant aspects of the broader question. Given the pervasiveness of the face-to-face small group setting across the various levels and structures of government and politics, leader activity in group deliberation processes would appear potentially quite important for a general understanding of political decision making. Of course, it is conjectural whether leadership activity in the management of meetings is related with other tasks associated with executive role performance. It is reasonable to hypothesize that leaders who are quite active in meetings maintain this style across task areas. Whether passive leaders in meetings tend to be passive across the board appears more problematic and warrants further research.

One important limitation of this study is that the data base is not longitudinal. Effects are hypothesized to occur over a protracted period of time in an individual's life. An optimal data set might include observations on individual elites (e.g, judges, presidents, governors) at points encompassing 10 to 20 years of continuous service in office, beginning by at least age 55. Thus the findings could reflect cohort, rather than aging effects upon behavior. A cohort explanation would imply that age cohorts differed systematically in some general disposition toward energy investment in job performance. Given that cohort effects are manifest through common attitudes and shared values and that the relevant attitudes for "energy investment in job performance" would encompass the so-called work ethic, it might be expected that older leaders in these data, coming of age in the 1930s and early 1940s, would display more, rather than less activity in leadership performance. Of course, cohort effects cannot be ruled out a priori. For a more general model of leader activity, it would be appropriate to consider cohort effects along with social and biological factors of aging.

Conclusion

In a recent review of the literature and state of knowledge on political leadership, Hermann (1986, 185) comments, "Of all the ingredients of political leadership, behavior has received the least attention." She goes on to note the complex problems that empirical research on leadership behavior must confront, and certainly the active-passive dimension is but one small piece of a much larger puzzle. However, the active-passive dimension, narrowly conceived and behaviorally defined, may provide a fundamental baseline from which to pursue more comprehensive empirical inquiry into leadership behavior.

We do tend to rely heavily upon the activity dimension in evaluating role behavior of people both in and out of official capacity, and elderly political elites are particularly vulnerable in this respect. Journalists bring it to our attention, increasing in visual form, when elderly senators tend to fall asleep during committee sessions or a president dozes off during meetings with other heads of state or international elites. Exacting attention to detail, factual recall, the capacity to absorb reams of technical material in late-night work sessions, vocal performance in face-to-face settings, or even lengthy televised debates are all task areas subject to the effects of biological aging. If we knew more about the effects of biological aging upon behavior, we might develop more reasonable expectations and reasoned evaluations of leader performance. Specific accommodations might be structured with respect to the length and timing of physiologically demanding task performance. Indeed, we might develop more realistic expectations of the various qualities of leadership (both costly and beneficial) likely to be associated with elderly elites.

The effects of aging on the active-passive dimension of leadership style were researched through direct observation of behavior for a sample of 24 mayors. The findings reveal that aging may have very important effects upon leadership, directly and through interaction with other political variables. It is quite reasonable to propose that the more encompassing the daily demands involved in a leadership role, the stronger the effects of aging on activity. Empirical effects should be weakest for part-time mayors of small communities and much stronger for governors and presidents. Of course, there are clear limits to generalization from these findings: the number of cases is small, the data are cross-sectional, and more comprehensive measurement of active-passive style would be desirable. Despite these limitations, the findings make excellent sense in theoretical terms and invite replication through longitudinal research design.

Notes

The research upon which this study is based was supported by a grant from the National Institute on Aging (IRO1AGO3779-01A1). The principal investigators included the author, Thomas Wiegele, and Samuel Hines. That project is described in Schubert, Wiegele, and Hines 1986. The comments on a presentation of the central ideas of this paper by participants at a 1987 conference on biology and politics, hosted by Jerzy Wiatr at the University of Warsaw, are gratefully acknowledged.

1. The only systematic bias in the selection of these councils was to include councils in each regional area with either older or younger age structures (i.e., median ages exceeding 55 or below 44). The outcome was 8 older, 6 younger, and 10 with "mixed" age structures. In both social, economic, and demographic characteristics of communities and sex, age, and occupational characteristics of council members, data were analyzed revealing that the sample councils and communities did not differ significantly from the population of the broader regions from which they were drawn (Schubert, Wiegele, and Hines 1986).

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IDEOLOGICAL CONSISTENCY AS A COLLECTIVE PHENOMENON

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Researchers ordinarily consider ideological consistency to be a characteristic of individuals; groups are considered to be ideological only if they are composed of ideologically oriented individuals. We show how a group as a whole can be characterized as exhibiting an ideological basis for its preferences even though many, or even most, of its members have preferences that are inconsistent with the supposed unidimensional ideological continuum. As an illustration, we show that the United States electorate of 1980 had collective preferences among the candidates Kennedy, Carter, Ford, and Reagan as if these preferences reflected an underlying left-right dimension among these candidates, despite the fact that a high proportion of individual voters had preferences among these candidates that did not fit the left-right dimension. In general, we show reasons why collectivities are likely to be more ideologically consistent than are the individuals composing them.

Despite the classic sociological recognition that the social whole is more than the sum of its parts (e.g., by Durkheim), the analysis of ideological consistency in collectivities has focused upon the ideological consistency of the individuals who compose them. For example, the well-known fact that individual voters in the United States by and large do not make choices in terms of a consistent left-right ideology (Axelrod 1967; Niemi and Weisberg 1984, 319–28 and references cited therein) has been interpreted to mean that U.S. society is not ideological in its choices.

We propose that "ideological consistency" may properly be considered to characterize collectivities as well as individuals. By choices being "ideologically consistent," we mean that group decisions are made *as if* the options were arrayed on a left-right continuum with voters choosing the alternative closest to their own preferences. In other words, when we speak of collective choices as being "ideological" or "ideologically consistent," we mean (following social choice terminology) that the preference structure is *the same* as that which would occur if voters had "single-peaked" preferences (Black 1958) along a single left-right dimension. We recognize that the term *ideology* has a plethora of meanings (see, e.g., Williams 1983 s.v. "ideology"), but the narrow meaning we give it is one with a long history of use and is directly relevant to the important empirical question of whether or not the U.S. electorate (and various subsets) can properly be characterized as responding to choices in left-right terms.

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VOL. 82 NO. 3 SEPTEMBER 1988

We demonstrate that even groups composed of individuals most of whose preferences are not ideologically consistent may be found to exhibit collective ideological consistency. Our central theoretical point is that there is a sensible notion of a collective ideology that is different from simply counting the proportion of individuals with ideologically consistent preferences or with some form of ideological self-identification, as is standard in the political science literature (see, e.g., Conover and Feldman 1984; Fleishman 1986; Hamill and Lodge 1986). We shall demonstrate that it is a "fallacy of composition" to believe that collective decision making will be ideological *only* when all or most members of the collectivity, as individuals, are ideological in their preference structure.

Berelson and colleagues (1954, 312) suggested that certain nonreducible properties of groups might be indispensable to the smooth functioning of the political decision-making process:

> Individual voters today seem to be unable to satisfy the requirements for a democratic system of government outlined by political theorists. But the system of democracy does meet certain requirements for a going political organization. The individual members may not meet all the standards, but the whole nevertheless survives and grows. This suggests that where the classic [political] theory is defective is in its concentration on the individual citizen. What are undervalued are certain collective properties that reside in the electorate as a whole and in the political and social system in which it functions.

We believe that *collective* ideological consistency is one property that may be important for the stability and coherence of a political system, because a political system that makes consistent and ideologically predictable choices may be more easily accepted as legitimate than one that appears inconsistent and unpredictable. We believe that collective ideological consistency is commonly found; that is, groups often possess the well-ordered preferences that make it possible to treat a collectivity as if it had coherent ideological preferences.

The coherence of group ideological preferences has important implications; for instance, many political groups (both manifest and latent) can be treated as if they were single entities by actors in the political process. Political candidates can choose positions in response to "group" preferences even though many or most members of the group are not strictly ideological. Similarly, parties can seek out candidates who reflect the "views" of the parties' constituencies. Thus, we can see how ideologically oriented representatives can nonetheless "faithfully represent" the view of an electorate primarily composed of nonideological voters (Feld and Grofman 1986a) and how newspapers and commentaries might sensibly talk about ideological shifts in the electorate, even though many or most voters never had coherent ideological views to start with.

Conceptualizing Individual versus Collective Ideological Consistency

The standard approaches to defining ideology in groups are methodologically individualistic in perspective, that is, they treat a collectivity as being ideological in the manner that, and to the extent that, its individual members are. For example, in the usual Guttman scaling approach (see Weisberg 1972 for a review), a set of choices can be said to conform to an underlying ideological basis of choice if there is an ordering of items such that each voter can be assigned a position on a continuum that completely determines his or her entire set of choices; essentially all individuals must be ideologically consistent for their choices to "Guttman scale."

Similarly, factor analysis, one common approach to ideological consistency, operates on a matrix of bivariate correlations (Harmon 1976; Weisberg 1974) that indicate the extent to which individuals have views that are coherent across choices. Ideological consistency is indicated by the amount of the individual variance that can be attributed to a single (or at most a few) factors.

Common sense might seem to suggest that there is no other way to define a group as being unidimensional than by saying that choice in accord with some single dimension is characteristic of members of the group *as individuals.* Yet we will show that it is perfectly possible to talk intelligibly about a group being ideological in its choices even when most of the group's members lack an ideological basis for their preferences. In fact, the converse is also possible; a group may fail to be ideological in its choices even if almost all of the individuals in the group make perfectly ideologically based choices. While there is some tendency for the extent of ideological consistency of individuals to affect the likelihood of collective ideological consistency (and the specific case of total ideological consistency of individuals necessarily results in group consistency), each form of ideological consistency can be high when the other is low (Feld and Grofman 1986b; Niemi 1969). We now turn to a precise statement of what we mean by individual and collective ideological consistency; then we consider some empirical evidence on ideological consistency in the U.S. electorate.

Definitions of Ideological Consistency in Individuals and Collectives

For a particular set of choices (alternatives) arrayed along a unidimensional continuum (e.g., left-right), we shall say that an *individual* has an ideological preference ordering with respect to that continuum if the individual has a most-desired alternative and prefers each of the other alternatives less as they are further from the "ideal" alternative in either direction. Such ideological preference orderings have been called "single-peaked" preference orderings (Arrow 1963; Black 1958), because they suggest that each individual has a utility function that is peaked at the ideal point and declines in both directions from that peak.[1] Once we specify such a unidimensional continuum and identify a set of alternatives ordered along it, then for every individual in a society we can in principle determine if that individual has preferences that are single-peaked with respect to the given continuum.

For the rest of this discussion, we will assume that a particular unidimensional continuum has been specified. A preference ordering will be said to be ideological if it is single-peaked with respect to the particular continuum that has been specified. Whenever we refer to a collective ordering we mean the preferences of the majority.

For example, consider the three alternatives A, B, and C along a continuum in that order. Each individual can be thought to have a preference ordering over that set of three alternatives; four of the possible orderings (ABC, BAC, BCA, and CAB) are consistent with single-peakedness for this continuum, and the other two possible orderings (ACB and CAB) are ideologically inconsistent with this continuum. To see that ACB is ideologically inconsistent with the continuum *ABC* note that if the individual had an ideal point at A, he or she must prefer B to C, since B is closer to A than is C along the *ABC* continuum. The argument that shows CAB to be inconsistent is analogous.

Suppose that 17 individuals all have single-peaked preference orderings: 8 have ABC, 5 have BCA, and 4 have CBA. Of the 17 voters, a majority (13) prefer B over C, a majority (9) prefer B over A, and a majority (9) prefer C over A. It is clear that B is the median of the voter ideal points; B is majority-preferred to both A and C. Furthermore, the group majority preference, which is B preferred to C preferred to A, is consistent with a hypothetical group utility function with ideal point at B and next highest utility at C. Such a function is single-peaked with respect to the continuum *ABC*.

Whenever *all* of the individuals have single-peaked preferences with respect to some continuum, it is well known that there is an alternative that is preferred by the majority of the individuals in the group to each and every one of the other alternatives. Furthermore this majority winner corresponds to the ideal alternative of the median voter in the group (Black 1958); and the majority preferences ordering of the group is itself single-peaked with respect to the underlying continuum (Arrow 1963).[2] Analogous results occur if *all* individuals have single-troughed preference ordering or if all individuals have what are called polarized preferences over every triple of alternatives (see Feld and Grofman 1986b; Plott 1976; Sen 1966), but we shall neglect these essentially technical complications and focus on single-peakedness.

When all individuals have single-peaked preference orderings the process of collective decision making is dramatically simplified. As long as all voters have such ideological preference orderings there cannot be any cyclical majorities (e.g., where A is majority-preferred to B, which is majority-preferred to C, which in turn is majority-preferred to A); also, the majority choice is consistent with all of the requirements of an ideal decision rule as set forth by Arrow (1963) in his classic consideration of social choice. However, while there are many situations where single-peaked preferences are common, there are few situations where everyone has single-peaked preferences.

Researchers have shown that a higher proportion of individuals with single-peaked preferences increases the probability that a group will have single-peaked preferences (Niemi 1969; Niemi and Wright 1986); however, there is no necessary connection between these two things. If there is even one individual with non-single-peaked preferences, then the group may have non-single-peaked preferences and there may be cyclical majorities. In the previous example, suppose that one of the voters with CBA preferences had changed to CAB preferences instead. There would then be a majority (9) who preferred A to B, a majority (13) that still preferred B to C, and a majority (9) that still preferred C to A. While there would still be 16 of the 17 individuals with single-peaked preference orderings, there would be a majority cycle, preferring A to B to C but C to A. More generally, as long as there are any individuals in the collectivity whose preference ordering is inconsistent with the posited ideological dimension, there is the possibility that the group will have a preference ordering that is inconsistent with the ideological continuum and even that the group will have no well-defined (i.e., transitive) preference ordering among alternatives.

Ideologically Ordered Margins in Collectivities

Collectivities indicate the *strengths* of their preferences by the sizes of their vote margins (see Feld and Grossman 1984). To the extent that collectivities are ideological, this ideology should be reflected by ideologically consistent regularities among the various vote margins between pairs of mutually exclusive alternatives, as well as by a single-peaked majority preference ordering at the aggregate level. Feld and Grofman (1986a, 103) define what it means for vote margins to be ideologically consistent; we expand upon that definition, consider how such ideologically ordered margins may arise, and specify an important new result about the aggregation of subgroups with ideologically ordered margins.

If all voters have single-peaked preference orderings over the continuum *ABC*, then everyone who prefers A to B must also prefer A to C. Therefore, if everyone in a group had single-peaked preferences, the total number of individuals voting for A over C would have to be at least as great as the number voting for A over B. Similarly, single-peaked preference orderings over the continuum *ABC* imply that everyone who prefers A over C also prefers B over C. Hence, if everyone in the group had single-peaked preferences, the total voting for B over C would be at least as great as the number voting for A over C. Thus the voting margins would have an ordering

$$m(\mathrm{A},\mathrm{B}) \leqslant m(\mathrm{A},\mathrm{C}) \leqslant m(\mathrm{B},\mathrm{C})$$

This can be illustrated in a matrix as shown below:

		Alternatives	
Alternatives	A	B	C
A	—	$m(\mathrm{A},\mathrm{B})$	$m(\mathrm{A},\mathrm{C})$
B		—	$m(\mathrm{B},\mathrm{C})$
C			—

The defining characteristic of this matrix of *ideologically ordered margins* is that in the area of the matrix above the diagonal the margins below and to the right of any cell are always at least as great as the margin in that cell. In other words, margins increase (or stay the same) in every row as we read from left to right *and* increase (or stay the same) as we move down in any column toward the main diagonal. Note that the ordering is the same whether the margins are expressed in terms of the absolute number voting for the row alternative over the column alternative or in terms of the vote margins by looking at the difference in number of voters between those voting for the row alternative over the column alternative and those voting for the column alternative over the row alternative. Also, it makes no difference if we express the matrix of margins in terms of absolute numbers or in terms of percentages.

For more than three alternatives, the implications of all individuals in the group having single-peaked preferences are the same as for the three alternative cases shown in the matrix above, that is, the alternatives can be represented in a matrix with the alternatives ordered across and down the side. The margins above the diagonal will follow the same pattern as in the matrix above, namely, increasing (or at least not decreasing) as one goes to the right along any row and as one goes down any column.

When margins satisfy the ideological margins condition with respect to a particular continuum, it can be seen that the majority preference ordering of the group as a whole must be single-peaked with respect to that continuum. This follows from the fact that there must be a first alternative, *i*, that is majority-preferred to the immediately following alternative, $i + 1$ (i.e., *i* is the first entry in the row whose margin over the alternative $i + 1$ is greater than a majority). Under these conditions the alternative before it, $i - 1$, must get less than a majority when paired against *i* (since *i* is the first to be majority-preferred to the next one, $i - 1$ must not be majority preferred to its following alternative); all alternatives before $i - 1$ (all margins above it) must have even lower margins (less than majorities). At the same time, since *i* receives a majority against the next alternative, $i + 1$, it must receive at least as great a margin (also a majority) against all following alternatives (all margins to the right). Thus, alternative *i* is majority-preferred to all alternatives before it and after it in the matrix. In like manner, with *i* deleted, we can find the next most preferred alternative in the majority preference ordering.

Ideological margins may arise even with many non-single-peaked voters. Indeed, in general, even situations where single-peaked orderings are relatively rare may still give rise to ideologically ordered margins, that is, margins of the sort that would arise if *all* individuals had single-peaked preferences. A simple example can illustrate this point.

Consider 17 voters such that there are 5 ABC, 2 CAB, 4 BAC, and 6 BCA preference orderings; the margins are ideologically ordered with respect to the continuum *ABC*, as shown in the matrix below, even though only 10 of the 17 individuals, those with the last two preference orderings, have single-peaked preferences with respect to the continuum *ABC*. Since 4 of the 6 possible preference orderings are single-peaked, this example represents a lower proportion of single-peaked preferences with respect to *ABC* than would be expected by chance alone (10 out of 17 vs. 4 out of 6); yet as shown in the matrix below, the ordering on margins is exactly the same as could occur if *all* voters were single-peaked with respect to *ABC*. Eleven voters have single-peaked preference with respect to the continuum *BCA*, but the group preference ordering BAC is not single-peaked with respect to *BCA*.

	Alternatives		
Alternatives	A	B	C
A	—	7	9
B		—	10
C			—

Note that if there are only three alternatives, ideological margins require that a majority of voters have single-peaked preferences, but for larger numbers of alternatives there may be ideological margins even if *no* individual has single-peaked preferences. Also note that (if we neglect ties) margins can only be ideological with respect to a single underlying continuum. As illustrated by the above example, the continuum along which the largest number of individuals will be single-peaked need not be the same continuum as that over which the group has ideological margins. In general, however, we do not expect to find many empirical examples of such a discrepancy between a group-based and an individual ordering-based perspective on what ordering is most characteristic of the group. The context for the individuals in a group is likely to produce tendencies toward single-peakedness on some particular continuum, and that continuum is likely to be the same one which forms the basis for the group's ability to satisfy the ideological margins condition.

The ideological margins condition is important because if it is satisfied for a set of subgroups composing a society, it must also be satisfied for the society as a whole. This is not true for single-peakedness; if we add several subgroups together, each of which has a single-peaked ordering (but without ideological margins), there is no guarantee that the combined group will have a single-peaked ordering.

The example below shows two collectivities that each have single-peaked preferences with respect to some underlying continuum (without ideological margins) that do not combine to produce a collective transitive ordering that is single-peaked with respect to that same underlying continuum. Consider three candidates on the continuum *ABC:*

Set X below has a hundred people and has majority preferences ordered ABC, which are single-peaked preferences over the continuum *ABC*, but X does not satisfy the ideological margins condition.

	A	B	C
A	—	60	55
B		—	51
C			—

Set Y below has a hundred people and single-peaked preferences CBA over the same continuum, *ABC*, but again does not satisfy the ideological margins condition.

	A	B	C
A	—	45	42
B		—	40
C			—

Sets X and Y combined have two hundred people and majority preference ordering CAB, as shown below.

	A	B	C
A	—	105	97
B		—	91
C			—

The preference ordering of the combined set X and Y is not single-peaked with respect to the continuum *ABC*. Thus, without ideological margins, the fact that subgroups of a collectivity each have single-peaked preferences does not ensure that the combined group will have single-peaked preferences.

On the other hand, it is easy to see that if we combine two subsets, each of which has ideological margins, the resulting combined group must have ideological margins; clearly, if each of the matrices has the margins ordered from left to right and top to bottom, summing them results in the same ordering. Furthermore, if each of the subsets has mostly ideologically ordered margins and the departures from ideologically ordered margins in each subset are different, combining the subsets may result in margins that are even closer to ideological ordering than was true for the subsets themselves.

We are not claiming that collectivities will inevitably have ideologically ordered margins. We do claim, however, that ideologically ordered margins will be more common than one might think by looking at the bewildering diversity among *individual* preferences. Furthermore, while many or even most of the individuals in a collectivity may have quite nonideological preferences, we expect that many collectivities will at least approximate the ideological margins condition and will have single-peaked preferences.

We believe that individuals do not make choices consistent with an underlying ideological continuum as often as collectivities do. The aggregate ideological consistency might arise in several ways. One possibility is that there is a relatively ideological elite whose preferences, combined with the self-counteracting "noise" of the nonideological masses, determine the group preferences. Another possibility is that there is a diffuse ideological tendency among a large proportion of the electorate that is sufficient to generate ideology in the aggregate; some individuals might have single-peaked preferences over some subsets of the alternatives while other individuals have single-peaked preferences over other subsets (cf. Feld and Grofman 1986b).

The Extent of Ideology in Margins

In the previous section, we showed that if subgroups have ideological margins, then the entire group necessarily has ideological margins and consequently must have a transitive preference ordering that is single-peaked. In addition, we suggested that if the margins of subgroups closely approximate ideological margins, then it makes it likely that the group as a whole will approximate ideological margins and consequently have single-peaked preferences. Here we will make the notion of "closeness" to ideological margins more precise.

To measure this notion of "strength" of ideological margins we simply measure the average difference between margins. If there are three alternatives, then there are three margins and three margin comparisons (AC − AB; BC − AC; BC − AB).[3] For three alternatives, the *average margin difference* is given by (AC − AB + BC − AC + BC − AB)/6, which reduces to

(BC − AB)/3. If margins satisfy the ideological margins conditions, each of these pairwise margins comparisons should be nonnegative; and even if there is "noise" the average difference in margins should be nonnegative.

If margins rapidly increase in each row or column as we move to the right, away from the main diagonal, or down, toward the main diagonal in some subgroup, then even when this subgroup is combined with other subgroups the pattern of ideological margins is likely to be maintained. The ideologically ordered pattern will tend to stand out over any random variations found in the other subgroups with which it might be combined. (Of course, any subsets drawn randomly from a group with "strong" ideological margins are likely to have ideological margins themselves.)

Recall that ideological margins are ordered so that they are nondecreasing as one moves to the right and down the portion of the margins matrix above the main diagonal. When several subgroups are combined, it should be clear that the margins for the entire group are merely the weighted average of the margins for the subgroups (weighted by the size of the subgroups). Thus if one or another subgroup has one pair of margins slightly out of ideological order (say, 3% in the wrong direction), then when it is combined with other subgroups having this same pair of margins in the right direction, the overall group will have this pair ideologically ordered. So even if some subgroups have slight deviations from ideological margins, there may be ideological margins when subgroups are combined.

Even if the collectivity as a whole does not have strictly ideological margins, it will still have a transitive single-peaked preference ordering so long as a margin above 50% is not followed (to the right or down) by a margin that is less than 50%. Thus as long as the few particular aggregate deviations from ideological ordering are not in this crucial region (around 50%) in the group as a whole, they will not interfere with the overall single-peaked collective preference ordering.

In the next section, we provide some illustrative empirical data about the extent to which a population and various subsets satisfy the ideological margins condition. Specifically, we analyze the preferences of U.S. society as a whole and a variety of subgroups within it vis-à-vis the top four candidates for president (Kennedy, Carter, Ford, and Reagan) prior to the 1980 elections. We find that most subsets approximately satisfy the ideological margins conditions with respect to the left-right continuum *KCFR*, which would be a left-right ordering by politically sophisticated observers. Moreover, as hypothesized, we find that the society as a whole, as well as virtually all its subsets, had (majority) preferences that were single-peaked with respect to this continuum, despite the fact that only barely more than half of the *individuals* in the society had single-peaked preferences with respect to this continuum *KCFR*.

An Empirical Example of Ideological Consistency of Groups

In January 1980 individual respondents were asked by National Election Studies (NES) interviewers to indicate their feelings towards each of the presidential candidates in the 1980 election by indicating a number from 0 to 100 (where 100 represents the most positive feelings). By using the feeling thermometer rating as a surrogate for utility (Feld and Grofman 1986b; Niemi and Wright 1986; Weisberg 1974; Weisberg and Grofman 1981), these data allow us to determine each voter's preference ordering over these candidates. The NES survey included a long list of candidates; for the present purposes, we confine our attention to the four candidates

Table 1. Margins of Preference among 1980 Presidential Candidates

	Candidates			
Candidates	Kennedy	Carter	Ford	Reagan
Kennedy	—	38.0	38.0	41.5
Carter		—	48.3	53.0
Ford			—	53.7
Reagan				—

Note: Entries are the percentage preferring the row alternative to the column alternative.

Source: NES 1980 candidate thermometer data (N = 1,481).

on the list who were best known: Carter, Kennedy, Ford and Reagan. Most respondents indicated that they knew of each of these candidates and had preferences among them.

Every one of the 24 possible linear preference orderings over these four alternatives was represented in the NES sample of voters. The eight preference orderings that were single-peaked over the continuum *KCFR* tended to be somewhat more common than each of the 16 possible non-single-peaked preference orderings; but overall, almost half (47%) of the preference orderings are *not consistent with* single-peakedness on the posited left-right continuum. No ordering is shared by more than 12% of the voters. The most common are RFCK (11.2%), FRCK (10.7%), and RFKC (7.9%), the first two of which are single-peaked; the least frequent orderings were RKCF (.7%), KFRC (1.2%), and RCKF (1.3%), none of which are single-peaked.

Despite the large proportion of non-single-peaked preference orderings, the margins of pairwise preferences conform to the ordering expected to arise from the situation where *all* voters have single-peaked preferences. Table 1 shows the margins of pairwise preference for the entire sample of individuals who indicated feelings about all of the candidates.

Table 1 illustrates how a society with a very large proportion of non-single-peaked preference orderings can, nonetheless, give rise to ideological margins like those that would occur if the entire group had single-peaked preference orderings. Even though a different majority of voters is on the winning side in each pairwise choice among alternatives, the majority of respondents in the NES sample have preference ordering FCRK, which is single-peaked with respect to the *KCFR* left-right ordering. Conformity of these margins to the requirements imposed by ideological margins would obviously be unlikely to occur by chance. Even though a near majority of those respondents in 1980 had preference orderings inconsistent with the underlying ideological continuum, the group margins still perfectly reflect that continuum.

We believe that this aggregate ideology arises from the fact that there is widespread recognition of the ideological continuum. In the next section, we show that there was general agreement on the placement of these candidates and that subsets with a particular self-identified position had preferences and margins consistent with that ideological position. Niemi and Wright (1986), analyzing the same data, find, as did we, that the society as a whole and various subsets of it exhibit far fewer majority cycles than might be theoretically expected. Niemi and Wright (1986) also found that the absence of cycles could not be attributed to the proportion of individuals having single-peaked preferences and concluded that ideological consistency was not an important factor underlying the transitivity of group preferences. We are suggesting that the proportion of individuals with single-peaked preferences does not measure the importance of an ideological continuum to a *group*. We agree, however, with Niemi and Wright's suggestion that widespread agreement on the goodness or badness of particular candidates also contributes to the absence of majority cycles. This line of approach is also discussed in Feld and Grofman (1986b) and is one that we have been investigating.

Ideological Self-Identified Subsets

Respondents in the January 1980 NES Election Survey were asked to identify their own position and the position of each candidate on a seven-point scale ranging from extremely liberal (1) to extremely conservative (7). The frequency distribution of respondent ideological self-identification is clustered around the "moderate" responses, with some tendency toward conservatism. We then analyzed the perceived positions of the candidates from the perspectives of the subsets of individuals in each ideological self-placement category. Due to the relatively small number of respondents in the end categories, in our analysis we combine the two extreme categories at each end of the ideological spectrum. Despite substantial variation among individuals, the mean assigned relative positions for the set of candidates differed as would be expected, with the candidates ordered from left to right as Kennedy, Carter, Ford, and Reagan by the members of each subset.

Having established that there is a general tendency among ideologically self-located subsets to perceive a left-right ideological ordering among the four major 1980 U.S. presidential candidates, we next determined whether the aggregate ordering of each subset was consistent with the self-identified ideological placement of subset members. Subsets with different ideological self-placement would be expected to differ in their rankings of the candidates if ideology was a relevant determinant of candidate preferences. Moreover, even if all individuals with a given ideological self-placement did not prefer candidates in terms of ideological proximity, we expect that each *set* of ideologically self-identified individuals would have an ordering consistent with that subset's self-placement on the left-right continuum.

If preferences satisfied the ideological margins conditions for the continuum *KCFR*, the expected directionality of marginal comparisons is as follows: KC < KF, KC < KR, KC < CF, KC < CR, KC < FR, KF < KR, KF < CF, KF < CR, KF < FR, KR < CR, KR < FR, CF < CR, CF < FR, CR < FR.

Table 2 shows information about the preferences of the extremely liberal, the somewhat liberal, the moderate, the somewhat conservative, the extremely conservative, and the self-identified nonideological. Table 2 also shows the margins of preference for each ideologically self-identified subset. The next to last column in lower part of Table 2 shows the number of ideologically consistent pairs of margins for each subset (out of the possible 14). As expected, each ideologically self-placed subset also has a majority preference ordering consistent with its self-placement; for instance, liberals have the majority preference ordering CKFR, moderates have the ordering FCRK, the somewhat conservative have the ordering FRCK.

Table 2 also shows our measure of strength of ideological consistency, the "average difference in pairs of margins" measure described in the previous section. Using that measure, we find that most ideologically self-identified subsets had pairs of margins which were, *on average*, highly consistent with our predictions, with the average differences shown in Table 2 ranging from 18% (for the somewhat liberal and the nonideological) to 33% for extreme liberals. The value of this measure could range (for four candidates) from −57% to 57%. In each subset the *average* difference in pairs of margins is comparable to, or considerably higher than, the *maximum* difference between pairs of margins in an ideologically inconsistent direction (shown in column 1 in Table 2).

The average strength of ideological consistency across all subsets is considerably higher than the maximum ideological

Table 2. Margins in Pairwise Choices among Individuals with Different Self-Identified Ideological Positions

Ideologically Self-identified Position	Maximum Difference between Pairs of Margins: Ideologically Inconsistent Direction (%)	Maximum Difference between Pairs of Margins: Ideologically Consistent Direction (%)	Matrix of Pairwise Margins of Preferences	Kennedy	Carter	Ford	Reagan
Extremely liberal or liberal	31	76	Kennedy	—	-5	23	46
			Carter		—	41	71
			Ford			—	40
			Reagan				—
Somewhat liberal	5	40	Kennedy	—	-11	1	25
			Carter		—	11	29
			Ford			—	24
			Reagan				—
Moderate	7	36	Kennedy	—	-22	-29	-23
			Carter		—	-4	5
			Ford			—	7
			Reagan				—
Somewhat conservative	19	62	Kennedy	—	-35	-54	-52
			Carter		—	-26	-19
			Ford			—	8
			Reagan				—
Conservative or extremely conservative	24	78	Kennedy	—	-35	-51	-59
			Carter		—	-37	-44
			Ford			—	19
			Reagan				—
Nonideological or no response	12	32	Kennedy	—	-24	-13	-6
			Carter		—	8	20
			Ford			—	8
			Reagan				—

Ideologically Self-identified Position	Ordering	Number of Ideologically Consistent Margin Comparisons	Average Difference in Pairs of Margins (%)
Extremely liberal or liberal	CKFR	11/14	33
Somewhat liberal	CKFR	12/14	18
Moderate	FCRK	12/14	18
Somewhat conservative	FRCK	12/14	23
Conservative or extremely conservative	RFCK	8/14	21
Nonideological or no response	CFRK	12/14	18

inconsistency between pairs of margins averaged across all subsets. Thus in general, when subsets are combined, we would expect even the most extreme "errors" in directionality between pairs of margins in each subset to be outweighed by the high average level of ideological consistency across all subsets. In other words, the strength of ideological preference calculations shown in Table 2 leads us to expect that almost any combination of subsets would tend to result in a collectivity with strong ideological margins and thus would almost certainly result in a transitive majority preference ordering single-peaked with respect to the posited left-right continuum.[4]

Despite the high aggregate level of ideological consistency in each of the ideologically self-placed subsets shown in Table 2, even within each ideologically self-identified subsets there is tremendous variation in *individual* preferences. Although each subset has a single-peaked majority preference ordering consistent with the subset's ideological self-placement, *individuals* within each subset do not show high ideological consistency. Among the very liberal, there are 16 distinct individual preference orderings and the modal preference (KCFR) is shared by only 37% of the individuals in the set. Only 65% have orderings that are single-peaked. Among the somewhat liberal, there are 21 different individual preference orderings and the modal preference (CKFR) is shared by only 15% of the individuals in the set. Only 52% have orderings that are single-peaked. Among the self-identified moderates, there are 22 different preference orderings, with the two modal categories having only 9% each. We find that 54% of this set of individuals have single-peaked preference orderings. Among the somewhat conservative, there are 19 different preference orderings, with the modal category having 9%. We find that 62% of this set have single-peaked preference orderings. Among the extremely conservative, the group margins are less consistent with ideology than among the extremely liberal; there are 21 different preference orderings, with the modal category having only 13% each. In this subset only 53% of the individuals have single-peaked preference orderings. Nonetheless, as before, the group as a whole has single-peaked preference ordering RFCK.

When those who claim to have no ideological position are examined, they show more variation among individuals but a similar aggregate result. The set of non-ideologically self-identified individuals exhibits *all* 24 difference preference orderings, with no more than 9% holding any one ordering. Less than half (46%) held preference orderings that were ideologically consistent with the posited left-right continuum. Nevertheless, as we anticipated, even the set claiming no ideological position as individuals *collectively* had a transitive majority preference ordering, CFRK, which was single-peaked with respect to the posited left-right continuum *KCFR*. That preference ordering indicates an aggregate position just slightly to the left of the self-identified moderates.

What we find striking about the data on the ideological consistency of individuals and subsets as a function of their self-reported position on a liberalism-conservatism scale are that (1) *no* subset had more than 65% of its members with single-peaked orderings and some subsets had only barely above 50% of their members with single-peaked ordering; yet (2) *each* subgroup had a single-peaked transitive majority preference ordering consistent with its self-reported ideology.

Other Types of Subsets

Liberals and conservatives will be found in different proportions among various population subsets. We looked at subsets defined by age, education, social class identification, and gender. For each subset (10 in all, because some variables were polychotomies, not dichotomies) we determined majority preferences.

Common sense might lead us to expect that some subgroups might more likely be collectively ideologically consistent than others. On the other hand, we might expect that the perception of a left-right ideological continuum as perceived by the population as a whole will be shared by the various subsets in the population. If this were true, we would expect difference not in the continuum shared by the groups but in the preference ordering held by various subsets for alternatives along that (shared) ideological continuum, of the sort we found for ideologically self-identified subsets. Also we might expect the fit of the ideological margin assumption to be less perfect for most subsets than for the society as a whole because of (1) idiosyncratic differences among subsets, that is, some subgroups, for instance, "Right to Life," might have a consistent ideology that is only partially correlated with a left-right ordering, and (2) the additional sampling error imposed by the smaller size of subgroup samples.

There are four different preference orderings manifested among these 10 subsets (for example, the working class and females have the ordering CFRK; the old and those who did not graduate high school the ordering CRFK; males, the college-educated and the middle class the ordering FRCK; younger and middle-aged voters and those with just a high school degree the ordering FCRK)[5] and two different majority winners. However, only the two subsets with ordering KCFR were not single-peaked. Thus 80% of the 10 subsets have single-peaked preference orderings, despite their dramatic differences in actual preferences, and all of the subsets had a transitive majority preference ordering.

The fact that virtually all of these subsets have, in the aggregate, single-peaked preferences with respect to the posited left-right continuum supports the hypothesis that the left-right perceptions of the nature of the presidential choices is diffused throughout the society, rather than found exclusively in one subgroup or another. Thus it seems clear that while ideology is not perfectly uniform across subgroups, some degree of common ideological orientation is present throughout the society. At the same time, the fact that the two subsets whose transitive ordering is not single-peaked with respect to the left-right continuum are the least well educated and the aged suggests these social groupings may reflect the societal ideological perspective less than others.

Conclusions and Implications

We have shown that only a bare majority of individuals in our 1980 sample have single-peaked preferences for presidential candidates, while 80% of all our subsets and the society as a whole have single-peaked preference orderings. Thus, as we had hypothesized, these collectivities are more ideological than the individuals who compose them. A major implication of our work is that the usual individual-based analysis of ideology will often miss a clear-cut ideology that may be present in the group as a whole. In particular, our approach helps to account for Inglehart's (1985) finding that there is stability of belief at the birth cohort (aggregate) level in contrast to seemingly random variations of beliefs over time at the individual level. In addition, our work helps to explain the common tendency for even the most knowledgeable observers to talk about groups as if groups had preferences and why it is reasonable for political observers to characterize political decisions as indications of the ideological tendencies of the electorate even though a high proportion of the electorate is not making decisions based upon ideology.

As we have shown, it is perfectly possible to talk intelligibly about a collectivity being ideological in *its* choices even when *most* of the collectivity's members lack an ideological basis for their preferences. Such a notion can be theoretically well defined without recourse to any "quasi-mystical" notions of emergent group properties. For the mass electorate to have single-peaked preferences (and something close to ideological margins), it is not necessary (or even critical) for most of its members as individuals to see the world primarily in ideological terms.

It is important to distinguish between the question whether members of a society agree with each other about preferences and the question whether they agree with each other about the continuum along which alternatives are to be evaluated. In other words, collectivities can agree, in the aggregate, on what we might call the "terms of the debate," while having great disagreement as individuals about what is the best choice. Moreover, agreement on that shared continuum at an *aggregate* level does not require a high degree of agreement at the *individual* level.

If there is a shared continuum, then the size of the margins conveys useful information about the nature of societal preferences along the left-right dimension. For example, in the situation previously described, in the society as a whole in January 1980 Carter did better against Ford than did Kennedy, even though both lost. Nevertheless, this is interpretable as indicating that the "median" in the population was—and a winning candidate would have to be—to the right of Carter.

The fact that the ideological nature of groups is not a simple function of the proportion of individuals who have single-peaked preferences suggests that the sources of ideological orientation must largely be attributed to tendencies toward ideological perceptions which may be only dimly realized in any single individual but which may cumulate across individuals (almost none of whom are perfectly ideological) so as to consistently provide an ideological cast to the decisions of the society as a whole and to virtually all of its subgroups. For example, there almost certainly is a tendency of the mass media to use ideological terms as shorthand labels, creating some diffusion of ideological characterizations even though many individuals are incapable of identifying choices in left-right terms and even though some who are capable of doing so do not make their choices entirely in those terms.

Furthermore, it has been shown (Feld and Grofman 1986b) that if a group has ideological margins it can be perfectly represented by a set of representatives all of whom have single-peaked preferences. Thus group ideology, as we define it, helps us account for an important puzzle about why legislative representatives appear to possess a well-defined ideology in left-right terms even though the voters they supposedly represent (and whose votes they compete for in order to win elections) by and large lack such an ideology.

Our empirical findings are only illustrative and we have not explored the factors underlying the emergence of a collective ideology. What we have accomplished, we believe, is to provide a *collective* perspective on the notion of ideology. If so, then the long-standing question of whether there is ideological consistency in *individuals* (e.g., Converse 1964, 1970) needs to be supplemented with a search for the roots of ideological consistency in *collectivities.*

Notes

This research was supported in part by a grant from the National Science Foundation, SES no. 85-06736, Decision and Management Sciences Program, and by NSF Grant BNS no. 80-11494 to the Center for Advanced Study in the Behavioral Sciences (CASBS), Stanford, where Grofman was a fellow in 1985-86. This research was partially conducted while Feld was a visiting professor at Dartmouth College, 1985-86. We are indebted to Leslie Lindzey and the staff of CASBS, Stanford and to Sue Pursche and the staff of the Word Processing Center, School of Social Sciences, University of California, Irvine, for manuscript typing and table preparation; to Dorothy Gormick and Roseanne Torres for bibliographic assistance; and to Howard Schuman for helpful comments.

1. Single-peaked preferences are identical to Coombsian I-scales (Coombs 1960). A sufficient but not necessary condition for all voters to have single-peaked preferences is to have voters choosing among alternatives in terms of which is closest to their ideal point along some line.

2. Note that the focus of our work is on ideological consistency, not merely on whether there is majority; of course, ideological consistency, as we have used the term, implies transitivity, thus it implies the existence of a majority winner. Preferences are *transitive* if, whenever alternative a is *preferred* to alternative b (denoted a P b) and also b P c, then a P c.

3. In general, for m alternatives, there are c_m pairwise comparisons of margins where

$$c_m = c_{m-1} + \left(\sum_{1}^{m} x^2 \right) - (m) = 2c_{m-1}c_{m-2}$$

$$+ m^2 - 1.$$

4. It should also be noted that the departures in Table 2 from an ideological ordering of margins are by and large ones that are least likely to interfere with single-peaked preferences for the collectivity. Respondents appear to distinguish more clearly among alternatives near to them than among alternatives far from them (a variant of the well-known "assimilation-contrast" effect). Thus, self-identified liberals did not prefer Ford over Reagan as much as we might have expected from comparisons of their margins; similarly self-identified conservatives did not prefer Carter over Kennedy as much as we might have expected. Reductions in strength of ideological margins caused by an assimilation-contrast effect of this sort do not generally affect the collective preference ordering. Since these inconsistent margins will differ across subsets, lessened ideological consistency of this sort in any given subset will tend to be cancelled out when we combine subsets composed of individuals with different ideological perspectives.

5. It should, however, be noted that these are not independent comparisons; each individual is included in five different subgroups (one for each variable). However, since many individuals do not have single-peaked orderings, it is possible for some subgroups to have ideologically consistent margins while others do not.

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A CULTURALIST THEORY OF POLITICAL CHANGE

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The most telling criticism of political culture theory is that it has coped very inadequately with political change. There is a good reason for this: the assumptions of the political culture approach in fact lead to the expectation of continuity. But continuity can be reconciled with changes, though only changes of particular kinds. The nature of political changes consistent with culturalist assumptions and with the culturalist expectation of continuity are here specified by hypotheses about (1) the effects of changes in social context, whether "normal" or involving abrupt discontinuity, and (2) the effects of attempted revolutionary transformation.

The political culture approach to building positive political theories and to political explanation has been with us since about 1960, and has been much described abstractly and much applied to concrete cases. The seminal works are Almond and Coleman's (1960) and Almond and Verba's (1963). Applications of the approach are covered comprehensively in a retrospective on the influence of their work by Almond and Verba (1979). Explications of it as a contender for paradigmatic status in political science, so to speak, occur in numerous works (e.g., Bill and Hardgrave 1973; Dawson and Prewitt 1969; Merkl 1970; Putnam 1973; and Pye and Verba 1965). My own use of the concept of culture, which I consider more precise than that of others, is discussed in the Appendix.

Political culture theory may plausibly be considered one of two still viable general approaches to political theory and explanation proposed since the early fifties to replace the long-dominant formal-legalism of the field—the other being political rational choice theory. Indeed, determining which of the two modes of theorizing and explaining—the "culturalist" or the "rationalist"—is likely to give the better results may be the single most important item now on the agenda of political science (Eckstein 1979a).

Whether or not it is advisable to take the culturalist road to theory depends above all on the ability to produce a cogent culturalist theory of political change: a theory consistent with the assumptions (postulates) of the approach and confirmed by experience. Criticisms of culturalist political theories certainly have emphasized the occurrence of certain changes in political structures, attitudes, and behavior and culturalist accounts of their occurrence in order to impugn the approach. Rogowski (1974), for example, has argued that political culturalists have been very offhand in dealing with change—that they have tended to improvise far too much in order to accommodate political changes into their frame-

AMERICAN POLITICAL SCIENCE REVIEW
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work. They have done so, he writes, to the point that they no longer have a convincing way to treat political change at all. His argument is directed at culturalist theory in general, but he singles out Almond's work with Powell (1966) as especially indicative of the sins that culturalists commit.

This argument—and others to similar effect—strikes me as cogent criticism of how culturalists have in fact dealt with political changes. Furthermore, difficulties accounting for change in general and for certain kinds of change especially seem to me inherent in the assumptions on which the political culture approach is based.

Difficult, however, does not mean impossible, nor implausible. It is quite possible to deduce from these assumptions a logically cogent account of how political change, and every kind of such change, occurs. My purpose here is to provide such an account, as remedy for the "ad hocery" Rogowski rightly criticizes.

The Postulates of Culturalist Theories and The Expectation of Continuity

The basic reason why a culturalist account of change is intrinsically difficult to construct (hence, why culturalists have in fact tended to waffle in explaining political change) is simple: the postulates of the approach all lead to the expectation of political continuity; they make political continuity the "normal" state.

The Postulates of Culturalism

To see why this is so we must first make explicit the fundamental assumptions from which culturalist theory proceeds—its "axiomatic" basis, so to speak. These assumptions unfortunately have been left implicit in culturalist writings. It is necessary to make them explicit if one is compellingly to specify what experiences are "normal" in a culturalist world and what conditions culturalist theory can and cannot accommodate.

The touchstone of culturalist theory is the *postulate of oriented action:* actors do not respond directly to "situations" but respond to them through mediating "orientations." All else either elaborates or follows from that postulate. What exactly, then, does the postulate assert?

"Orientations to action" are general dispositions of actors to act in certain ways in sets of situations. Such general dispositions pattern actions. If actors do not have them, or if orientations are ill formed or inconsistent, actions will be erratic: patternless, anomic. The idea of "orientations to action" follows a particular psychological stimulus-response model: not the simple "single-stage" behaviorist model in which nothing "subjective" intervenes between the experience of situations and responses to it (actions) but "mediational" models in which responses to stimuli (actions in situations) are considered results both of the experience of objective situations and actors' subjective processing of experience. "Orientations" do the processing. We may call them, as did Bentley, soul-stuff, or mind-stuff. The critical methodological task of studies based on such models is, of course, to penetrate reliably and with validity into the subjective.

Orientations are not "attitudes": the latter are specific, the former *general,* dispositions. Attitudes themselves derive from and express orientations; though attitudes may, through their patterning, help us to find orientations. If orientations frequently occur in collectivities they may be called "culture themes," as by Mead and Metraux (1954). Pye (Pye and Verba 1965) has distinguished four sets of such "themes" that he considers useful for making cultural comparisons on the societal level: trust-distrust, hierarchy-equality, liberty-coercion, parochial-national identifications. Putnam (1973) considers the theme of conflict or its counterpart, harmony, critical for cross-cultural analysis. These "themes" exemplify how "orientations" are general dispositions that pattern sets of actions and sets of specific attitudes. It is conventional to regard orientations as having three components: cognitive elements that, so to speak, decode experience (give it meaning); affective elements that invest cognition with feelings that "move" actors to act; and evaluative elements that provide goals toward which actors are moved to act (Pye and Verba 1965).

The assumption of oriented actions would be vacuous without the addition of a second postulate, which we might call *the postulate of orientational variability:* orientations vary and are not mere subjective reflections of objective conditions. The significance of this postulate lies particularly in this: if the processing of experiences into actions were uniform—if it were fixed at the biological level or if it always involved "rationalist" cost-benefit calculation—then mediating mind-stuff could simply be left out of theory. In Hempel's terms, we would only need to know "initial conditions" (situations, structures) to explain actions, since we already know the universal covering law needed to complete an *explanandum.* No doubt ingenuity is required in relating conditions to actions via uniform orientations: the rational choice theories we have provide more than enough cases in point. But this does not alter the logic of the argument that without orientational variability we remain in a strictly behaviorist world. Similarly, if actions are merely "superstructural," we manifestly need only to know situations to explain actions. In that case, only the explanation of deviant cases (like false class consciousness) would require the use of mediating variables.

If orientations are not inherent in actors but variable, then something that is variable must form them. And if orientations are not simply subjective reflections of varying objective situations, then the variable conditions through which they are formed must themselves be cultural. Orientations are not acquired in some automatic way; they must be learned. Thus, a *postulate of cultural socialization* must hold if the first and second assumptions hold: orientations are learned through the agency of external "socializers." The repertoire of cognitions, feelings, and schemes of evaluation that process experience into action must be imparted by the socialized carriers of culture. The process can be direct, by "teachers" who are culturally variable actors; or it can occur indirectly simply through the experience of variable cultures.

"Rationalist" theorists do not, of course, reject the notion of political socialization. That would be silly. What divides culturalist and rationalist theorists here involves the issue of late-in-life learning, or resocialization.

In regard to that matter, culturalists proceed from a *postulate of "cumulative" socialization.* This means two things. First, although learning is regarded as continuous throughout life (which is not likely to be questioned) early learning—all prior learning—is regarded as a sort of filter for later learning: early learning conditions later learning and is harder to undo. Second, a tendency is assumed toward making the bits and pieces of cognitive, affective, and evaluative learning form a coherent (consistent, consonant) whole.

The postulate of cumulative learning provides the culturalist account of how two fundamental needs of actors in societies are satisfied: the need for economy of action and the need for predictability in interaction. Life would hardly be bearable, even possible, if one had to think out every action, taking into account all pertinent information and lack of information. Orientational schemata thus save virtually all decision costs. Social life, similarly, would hardly be possible without reliable preknowledge of others' actions and of the effect of one's own actions on those of others. Without such preknowledge social life would tend to be entropic. As Crozier (1964) has cogently argued, "uncertainty" of action also begets power—arbitrary power.

Both economy of action and predictability in interaction are diminished to the extent that individual orientations are inconsistent and that early learning may readily be undone. These conditions have effects similar to a lack of orientations to actions and of socially shared orientations altogether. They lead to erratic, incoherent behavior by individuals and in social aggregates: anomie in the former; the absence of anything like a stable *conscience collectif* in the latter.

It should be pointed out that the culturalist solution of the problems of economy of action and social predictability is not a unique solution, however plausible it may seem. Thus, in the rationalist perspective, economy of action is provided by "ideologies" or by the sensible delegation of decision-making powers (Downs 1957). The fixity required for predictability in social life follows from the very fact that rational choice is considered a fixed disposition. If this is so, one can anticipate the actions of others and adjust one's own behavior to the anticipation. Social predictability may also be achieved through rationally formulated and enforced contractual arrangements or general legal rules. (It should be apparent that the two accounts of economy of action and social predictability provide a good basis for evaluating the relative power of culturalist and rationalist perspectives.)

To summarize, "cultural" people process experience into action through general cognitive, affective, and evaluative predispositions; the patterns of such predispositions vary from society to society, from social segment to social segment; they do not vary because objective social situations or structures vary but because of culturally determined learning; early learning conditions later learning and learning involves a process of seeking coherence in dispositions. And this is so in order to "economize" in decisions to act and to achieve predictability in social interactions.

The Expectation of Continuity

When the postulates of the political culture approach are made explicit, it should be evident why political culture theorists *should* have difficulties in accounting for political change. The assumptions of culturalist theory manifestly lead to an expectation of continuity, even in cases of changes in the objective contexts of political actions.

The expectation of continuity in aggregate (and individual) orientations follows most plainly from the assumption that orientations are not superstructural reflections of objective structures, but themselves invest structures and behavior with cognitive and normative meaning.

Cultural continuity also manifestly follows from the assumption that orientations are formed through processes of socialization. To the extent that socialization is direct (by precept), generational continuity must occur, the socializers being formed, "cultural men." To the extent that socialization is indirect (by experience), generational continuity still follows; experience with authority occurs first in the family, then in schools, where unformed children encounter formed adults. In either case, what is true of one generation should continue substantially to be true in the next. This applies as much to cultural divisions in a society as to more general culture types and themes—if any exist in the first place.

This, incidentally, makes the political culture perspective quite compatible with the finding that political regimes typically are short-lived (Gurr 1974).

The expectation of continuity in political cultures follows, most obviously, from the assumption of orientational cumulativeness, namely, that earlier learning conditions later learning and that actors tend to seek orientational consonance. The first allows some room for adult socialization and resocialization—but not much. The second makes unlikely the internalization of piecemeal orientational change that might increase dissonance.

But if change in culture patterns and themes were categorically excluded, political culture theory must immediately be thrown out as obvious nonsense: changes happen, including cultural changes. The saving grace of culturalist theory here is that continuity is, so to speak, an ideal-typical expectation—one that holds in an *abstract*, parsimonious cultural world. It is an expectation akin to that of inertia in the Galilean conception of motion. Physical inertia does not rule out changes of direction or rest, acceleration, and deceleration. It does make such phenomena depend on contingent factors that may or may not impinge on objects in motion. Continuity is the inherent (lawful) expectation and so, therefore, is resistance to change of motion: exceptionally great forces are needed to induce great changes in direction or velocity. The notion of continuity as inertia in motivations (the psychological counterpart of physical motion) thus opens the door to culturalist accounts of change.

Through that door, however, the tendency toward improvised, post hoc accounts of political change may enter—may be bound to enter. If one's preferred theoretical approach implies a strong bias toward the continuity of culture or resistance to cultural change, then it is always tempting to extemporize theory-saving "special" conditions, or adjustments in concepts or theory to handle occurrences of change—especially major change. If, say, theoretical difficulties arise from emphasizing early socialization, then why not just relax that emphasis and assign more scope for late socialization or adult resocialization? If the assumption of a tendency toward orientational consonance makes it awkward to explain certain observations, then why not simply posit more toleration for dissonance? Or why not redefine consonance? In that way, however, one is likely to end with the term continuity meaning nothing more than "not completely (or instantaneously) changeable"—which drains the term of all reasonable meaning. This is exactly the point of Rogowski's criticism of how culturalists have in fact accounted for political change.

The remedy is to develop an explicit general culturalist theory of change, consistent with culturalist assumptions, in order to prevent ad hoc tinkering with culturalist postulates and their implications. Such a theory should state, prior to explanations of specific changes, the characteristics of change that the political culture approach can logically accommodate and those that do not fit its constraints.

To formulate such a theory, I will consider two broad types of cultural changes: those arising "naturally" from changes in situations and structural conditions and those that result from "artifice"—deliberate attempts to transform political structures and behavior.

Situational Change

Pattern-Maintaining Change

Actors must often face novel situations with which their dispositional equipment is ill suited to deal. The world changes or presents us with experiences that are unfamiliar for other reasons (say, the penetration of peasant societies by market forces). The unfamiliar is encountered routinely in maturation, as one proceeds from family to school, from lower schools to higher ones, and from schools to participation in adult institutions. At the level of society and polity, novel situations arise from internal "development," however development may be conceived. Novel situations also arise from socially internal discontinuities (economic crises or political disruptions, like those caused by governmental instability or collapse, or from changes brought about by protest movements), or from externally imposed changes. Immigration brings actors into unfamiliar situations. So does internal migration and social mobility. The encounter of novel situations will, no doubt, occur much more frequently among individuals than on the macro level, but it also occurs in groups and societies.

Novel situations may be short-lived results of ephemeral upheavals. In that case no cultural adjustments are needed, nor are they likely to occur. What, however, should one expect if such situations persist?

If cultures exhibit inertia then it should be expected that changes in culture patterns and themes will occur so as to maintain optimally such patterns and themes; that is to say, changes in culture are perfectly consistent with culturalist postulates if they occur as adaptations to altered structures and situations and if the function of change is to keep culture patterns in existence and consonant. "Pattern maintenance" (Parsons' concept) can take that form just as well as strict cultural continuity.

The French have a half-facetious adage for this sort of pattern maintenance: The more things change, the more they remain the same. The saying no doubt fits (used to fit?) France. The pragmatic masters at pattern maintaining change, however, have been the British. Tory concessions to British working class voters and interests are the usual case in point. Their function—sometimes "latent" but in the case of Disraeli's Tory democracy quite explicit—was to maintain Tory hegemony in the face of considerable sociopolitical change through the maintenance of as much as possible of what the Young England Circle considered the feudalistic virtues: the disposition to defer to one's betters and action by the betters on behalf of the lower orders. The point applies to reforms of the suffrage and also to the less well known role of the Tories in the evolution of the British welfare state, which Tory governments not only have kept virtually intact but much of which they pioneered.

An alternative to pattern-maintaining change is to subject unfamiliar experience to procrustean interpretation in order to obviate cognitive or normative change. "Perceptual distortion" has turned up frequently in experiments on how individual cognitive dissonance is handled (see Brehm and Cohen 1962). We know at least a little about the same way of dealing with the unfamiliar on the political macro level. To give just one example: party political elections in Northern Nigeria were initially regarded as a version of long-familiar elections to chieftaincy, in which the "candidates" were a small number of ascriptively defined eligibles (Whitaker 1970). The extent to which perceptual distortion can be adaptive to unfamiliar experience no doubt is highly limited. However, where institutions like elections to chieftaincy exist in traditional cultures, the adaptation of dispositions to other kinds of elections should be easier than in other cases.

Change Toward Flexibility

Highly modern societies have traits that make it especially likely that actors and aggregates of actors will frequently confront novel situations. Social mobility, vertical and horizontal, is the most obvious cause. Because any changes in dispositions are costly (dysfunctional) in the culturalist perspective, one should expect as a correlate to the expectation of pattern-maintaining cultural change, that the more modern societies are, the more the elements of their cultures will be general, thus flexible. No doubt there are considerable limits upon how general and flexible orientations can be and still perform their functions of making experience meaningful, actions economical, and interactions predictable. In more modern societies one should not expect culture to change as readily as situations and structures. Situational and structural change tend to occur with great frequency and rapidity in modern societies, and the assumption of orientational inertia postulates resistance to frequent, swift reorientation. Rather one should expect that the rigidity of cultural prescription will relax, so that culture can accommodate much social fluidity.

The tendency toward cultural flexibility can be regarded itself as a way to maintain cultural patterns and themes. As societies become more changeable, the elements of culture increasingly become "forms" that can subsume a variety of "contents." It is probably no coincidence that some sociologists early in the twentieth century (especially Simmel [1950]) adapted the Kantian distinction between form and content to social analysis. Durkheim argued much the same point directly. In early societies, he wrote, "the collective environment is essentially concrete . . . [and] the states of conscience then have the same character." ("Culture" is not a bad translation of his notion of a *conscience collectif.*) As societies develop, the "common conscience" is obliged to rise above diversity and "consequently to become more abstract. . . . General ideas necessarily appear and become dominant" (Durkheim 1960, 287–91).

I want to make three other points pertinent to the expectation that cultural abstractness and flexibility will grow with social development. First, the disposition to act "rationally" introduces just the kind of general and flexible culture trait that inherent social fluidity requires. (Durkheim [1960] already associated rational attitudes and behavior with the abstractness of thought necessary in highly developed societies.) The rationalization of modern life—which Weber considered to be its governing trait—thus may be an accommodation to structural conditions rather than, contra Weber, their underlying cause.

Second, the obviously difficult problem of finding a proper trade-off between two warring imperatives in modern societies, that of cultural flexibility and that of cultural fixity, is bound to be a practical difficulty, not just a theoretical one. Reconciling fixity with flexibility, abstractness, and formality may be a crucial element in what has widely been perceived as growing malaise in highly modern societies. Anomie will follow not only from lack of internal guides to action but from guidelines too general and loose to serve in the relentless particularity of experience. Highly modern society thus may be intrinsically acultural and, for that reason, transitory or susceptible to *surrogates* for culture—including cults and dogmas.

The expectation of cultural flexibility, finally, should apply to *all* highly modern societies. It thus pertains to polities initially based on rigid dogma (like communist societies) that have successfully pursued modernization. In such societies, the first expectation, that of cultural inertia, should hold. Old culture should resist new dogma. The expectation of pattern-maintaining change (or perceptual distortion) should hold as well. So one should expect also that as culture changes in such societies, it will change toward greater flexibility—and therefore to reinterpretations of dogma that make it increasingly pliable.

Cultural Discontinuity

Contextual changes can be so considerable or rapid or both that neither pattern-maintaining changes nor changes that gradually relax cultural rigidity to deal with social fluidity are possible. Rapid industrialization is the case in point usually cited. Changes resulting from war or from the formation of new polities also generally involve upheavals in social contexts. Such upheavals may result as well from economic traumas like the great inflation of 1923 in Germany (which led to far greater social disruption than the Great Depression—or possibly even the Black Death). And traumatic change sometimes strikes special segments of society rather than the whole.

We must deal, therefore, with social discontinuity, as well as "normal" change. Culturalists have tended either to avoid the matter or, worse, to treat cases of social trauma simply as "deviant cases" in which the theoretical constraints of their perspective are off—not least, the expectation of cultural inertia.

Obviously, traumatic social discontinuity will have cultural consequences different from contextual stability or less rapid, less pervasive change. Even in such cases, however, we may not simply improvise. If the assumptions of culturalists are correct, then traumatic social discontinuity should have logically expectable consequences, no less than other change.

The one consequence of social trauma absolutely precluded by culturalist assumptions is rapid reorientation. Social upheaval may overcome cultural inertia, but if so, actors should be plunged into a collective infancy in which cognitions that make experience intelligible and normative dispositions (affect, evaluative schemes) must be learned again, and learned cumulatively. No culturalist may expect, for instance, a democratic political culture to form, in a few short years, in a society like Germany after World War II, or "national" orientations to form rapidly in postcolonial tribal societies. Instead, changes in political cultures that occur in response to social discontinuity should initially exhibit considerable formlessness. For *formlessness* one may substitute other terms, like Durkheim's *anomie* or Merton's *deinstitutionalization.* The essence of the matter is that culture loses coherent structure. It becomes highly entropic.

The idea that rapid, large-scale contextual changes are personally disorienting and culturally disruptive is hardly new. Lipset (1960) argued a generation ago that rapid economic development is associated with political "extremism" ("anomic protest movements" like anarchism and syndicalism), despite the fact that high levels of such development are related to political stability. Huntington (1968) later made much the same point, and Olson (1963) has probably developed it most cogently.

To say that formlessness under conditions of socioeconomic discontinuity should be "considerable" is not mere hedging. Cultural entropy can never be complete. If it were, no patterned action or interactions would be possible at all. In any case, social discontinuity never is total—intimate social units, like the family, survive the greatest upheavals (may, indeed, be strengthened by them, as refuges of predictable order); so too do structures that are supposedly merely instrumental—for example, bureaucracies. As well, if learning is cumulative, older people should exhibit a good deal of orientational inertia even when traumatic socioeconomic change occurs. We may surely suppose that the more ingrained orientations are and the more they are consonant systems, the less susceptible they are to "disorientation"—the more mechanisms like perceptive distortion will be used to invest experience with accustomed meaning.

Governmental authority will, of course, survive cultural discontinuity. In fact, it is likely to become more powerful to the extent that internalized dispositions cannot govern actions and interactions. How then do people act politically if political culture is highly formless?

We can get useful clues to answers from the growing literature on an analogous experience: how children adapt to novel situations that they enter in highly discontinuous ways: going to school, for instance, or going from one to another type or level of schooling. Much of the literature on this subject (like Wakeford 1969 and Woods 1979) has been informed by Merton's (1949) path-breaking study of the bases of deviant behavior, which dealt in general terms with behavior under more or less "anomic" conditions.

Under conditions of cultural discontinuity, conformity with authority is still likely to occur, but it will tend to have certain characteristics. In Merton's technology, it will tend to be *ritualistic* or else *self-serving* (opportunistic and of dubious morality, as general culture defines morality). Ritual conformity is compliance without commitment. One does what the rules or rulers prescribe, not for any discernible reason but (quoting from a lower-class British pupil interviewed by Woods) "because I behave meself . . . I just do what I'm told . . . [I] ain't got much choice." Conformity of this sort may be supposed to occur frequently in cases in which the former political cultures and subcultures prescribed high compliance ("subject cultures," as Almond and Verba called them). Self-serving, opportunistic conformity bends norms and rules for private advantage—including that of getting ahead in the competition for political power. Charles Dickens observed a lot of that sort of behavior in his travels in America as he reports them in his *American Notes.* Thus, in regard to a very successful businessman, " 'He is a public nuisance, is he not?' 'Yes sir,' . . . 'And he is utterly dishonest, debased, and profligate?' 'Yes, sir.' 'In the name of wonder, then, what is his merit?' 'Well, sir, he is a smart man' " (1957, 246). I mention Dickens because one should especially expect "smart" conformity in immigrant societies or immigrant segments of society, where (as in schools) discontinuity occurs through movement into an unfamiliar but intact culture. Perhaps one should expect it even more in cultures greatly unsettled by upheaval. Thus, Burke presciently remarked (in 1790) that when cultural constraints are off, "the worst rise to the top" (1923, 45).

More commonly than conformity, one should expect what Merton called *retreatism* under conditions of cultural discontinuity. Retreatism involves withdrawing from the "alien" larger society into the smaller, more familiar worlds of family, neighborhood, village, and the like. In Almond and Verba's scheme of concepts, it should show up as increased "parochialism." In the small worlds of schools, retreatism tends to involve self-imposed isolation—for instance, into remote places and daydreams or what Woods calls removal activities—"unserious pursuits which are sufficiently engrossing . . . [to make participants] oblivious for the time being of [their] actual situation"—or both.

Rebellion against, and intransigent resistance to, authority are also likely responses to the experience of cultural decay. A voluminous literature links social, economic, and political discontinuities to political violence—from Marx to Moore and Skocpol. Rebellion and intransigence, however, are always likely to be costly and call for much energy; retreatist behavior into parochial worlds or ritualistic conformity are thus more likely, especially where governing power—if not authority—is strong.

What should follow over time from contextual and cultural discontinuity? If economy of action and predictability indeed are imperatives in individual and collective life, one should expect new culture patterns and themes to emerge. But if dispositions are formed by cumulative learning, they should emerge only slowly (over generations) and, in the transitional period, at great costs resulting from raw power, withdrawal, and (because of withdrawal) forced mobilization and rebelliousness against it. Thus, the process of reformation of political cultures should be prolonged and socially costly. This is all the more likely to be the case if parochial units remain intact refuges from discontinuities in society, economy, or polity.

The expectation is logical also if older people, as is likely, cling to long-fixed dispositions even in face of strong forces that might unsettle inertia. We might thus posit as a general expectation that in the process of cultural reformation considerable age-related differences should occur. In fact, age, in cases of pronounced discontinuity, might even be expected to be a major basis for subcultural differentiation. If indeed this were found to be so, the cultural perspective upon theory would be enormously strengthened over alternatives. Empirical work pertinent to the expectation, however, is oddly lacking; and as culturalists have built adult learning increasingly into their approach in order to accommodate ill-fitting facts, the incentive to inquire into age-related cultural differences, in both established and transitional contexts, has regrettably declined.

I want to make another point about the reformation of dispositions and culture patterns, more briefly. As the young should be more susceptible to reorientation than the old, so one should expect to find in social macrostructures particular segments that have traits especially conducive or susceptible to reorientation. By "conducive traits" I mean structural or dispositional traits readily accommodated to new culture patterns or, indeed, anticipations of them. In Western traditional societies, for instance, there always existed a large island of achievement in a sea of ascription—the celibate clergy, which hardly could be ascriptively recruited. The clergy, in fact, played a considerable role in the emergence of modern political institutions—despite its stake in the distribution of traditional privileges. Similarly, socially "marginal" groups—groups that occupy the fluid interstices of established cultures—should be highly susceptible to reorientation, thus "vanguards" in the reorienting of unsettled societies. There is a good deal of literature making the case that this is indeed so (e.g., Rejai and Phillips 1979 and Wolf 1973).

Political Transformation

By *transformation* I mean the use of political power and artifice to engineer radically changed social and political structures, thus culture patterns and themes: to set society and polity on new courses toward unprecedented objectives. Transformation, typically, is the objective of modern revolutions. It can also be the objective of military conquerors and of nation builders or other modernizers. Revolutions, however, provide the most unambiguous and dramatic cases. I will therefore confine my remarks to them—though what is said about them should also apply to transformation attempted in other ways.

Hannah Arendt (1963) undoubtedly was right in arguing that attempts at revolutionary transformation are distinctively modern—that revolutions as we think of them (not mere rebellious attacks on authorities or their actions) begin with the French and American revolutions. As long as political and social structures were considered divinely ordained, or natural, or simply the ways of a folk, the idea of their deliberate transformation hardly could occur. "History" then could only be endless repetition or an intrinsic progress toward a preordained end. Societies and polities could no more be "transformed" than the heavenly bodies set upon new orbits. One of the decisive traits of modern societies then is the belief that a "new beginning"—a felicitous and not redundant expression—could be made in political and social life.

Initially, making a new beginning did not seem to call for much artifice—no more, perhaps, than a proper constitution. Achieving liberty or equality throughout society simply called for setting polities and societies on their inherently right course—right, given human nature. For reasons not necessary to sketch in the age of the "God that failed," *really* making revolution—not seizing power but the accomplishment of transformation—came increasingly to be seen as a task, and a difficult task, for political artificers. Unfortunately, systematic studies of that process are few, although the exceptions often have been notable: for instance, Massell's study of Soviet attempts to bring Soviet Central Asia into modernity (1974) and Kelley and Klein's study of the effects on inequality of the Bolivian Revolution of 1951 (1981). Inquirers into revolution still are hooked on the issue of their etiology.

Since revolutions are themselves major discontinuities and since they generally occur in periods of social or political upheavals, not least governmental breakdown (Edwards 1927; Brinton 1965), the expectations listed in the preceding section should apply to transformation. But I want to state here some expectations that follow from the culturalist perspective especially for processes of revolutionary transformation. Intrinsic interest and contemporary relevance aside, these processes seem to me especially critical for evaluating culturalist theories and their bases. After all, transformative processes involve not only adjustment to necessity but also the deliberate engineering of great change, and they are typically backed by great power and control.

As a first expectation we may posit that revolutionary transformation is strictly impossible in the short run. Revolutions certainly bring upheaval. They may also be expected to bring about movement in the direction of their professed goals by readily accomplished actions—instituting wide suffrage, kicking out the landlords and redistributing land, ending feudal privileges and obligations, and the like. But if discontinuity begets "formlessness" of culture, then revolutionaries can hardly do much to reorient people in the short run (say, in a generation or so). Reorientation is, of course, the less likely the more intact is the prerevolutionary culture: the more it provides parochial refuges from transformative power or institutional centers of resistance to it. But even if revolution only reflects discontinuity instead of engendering it, the expectation stated still should hold.

If the conventional norms and practices of political life are disrupted by revolution, what can be put in their place? We may posit the answer that revolutionary transformation will initially be attempted by despotic or legalistic means. What, after all, can "order" societies and polities in place of conventional, internalized culture? Only brute power, or else the use of external legal prescriptions as a surrogate for internal orientational guides to behavior. "Revolutionary legalism" was in fact a device used early after the Bolshevik seizure of power, and it overlapped a good deal (even before Stalin) with attempts to "storm" society (especially its more backward parts) with head-on "administrative assault." Neither, according to Massell (1974), accomplished much toward the realization of transformation; responses to it, he writes, included "avoidance," "selective participation," "evasion," "limited retribution," and "massive backlash."

"Legalism," it might be noted here, is likely to be a general response to massive cultural disruption, whether revolutionary or situational or both. Indeed, it can become, in highly unusual cases, a persistent surrogate for normative culture—indeed, a culture form. I have argued this elsewhere (1979b), defining "legalist" cultures as cultures in which legal rules are widely known, such rules are widely used (instead of justice or prudence) to justify political standpoints or decisions, legal actions are the normal mode of dealing with conflicts and disputes, and therefore laws deal in highly detailed—if possible comprehensive—ways with social interaction and tend to be punctiliously adhered to. Durkheim (1960) argued the even more general, related proposition that in the course of development civil law (which regulates social interactions) constantly grows, while criminal, or restitutive, law declines. His argument makes sense if indeed development "loosens" normative cultural prescription, as I argued, and lessens cultural similitude, as Durkheim argues.

The case I used to make this argument is contemporary West Germany. That, we should note, also is the case Rogowski (1974) mainly relies on to argue that reorientation *can* occur rapidly—the crucial point in his critique of culturalist theory. Rogowski seems to me to miss the real import of "deviant cases"—that through their very abnormal characteristics they can be used to shed light upon the factors that condition typical cases.

What of the long-run prospects of revolutionary transformation? I suggest the expectation that the long-run effects of attempted revolutionary transformation will diverge considerably from revolutionary intentions and resemble more the prerevolutionary condition of society. The expectation is not that little change in "content" will occur: in who holds power, gets privilege, and so on. No inevitable Thermidorean Reaction is posited. The argument is somewhat less categorical: reconstructed culture patterns and themes will diverge widely from revolutionary visions and will tend to diverge from them in the direction of the patterns of the old society and regime. The degree to which the expectation holds obviously depends on the extent to which the old culture was already in disarray.

Several points made earlier lead to this expectation. Culture must still be learned on a comprehensive scale, as in all societies; and although revolutionary teaching can no doubt play a considerable role in shaping the young, it can hardly replace socialization in small parochial units. Nor are teachers or role models likely to be, extensively, the sort of marginal individuals who are steeped in revolutionary dogma as a surrogate for convention—or people for whom the revolutionary vision has much meaning at all. Sheer cultural inertia will also play a role in the process of revolutionary decay; so will the tendency toward turning change into pattern maintenance—perhaps by a progressive transformation of revolutionary visions into mere revolutionary rhetoric; so—to the extent that the new rulers succeed in modernizing—will the tendency of modern cultures to be general, abstract, and (especially pertinent here) flexible; so will "retreatist" and "ritualist" responses to discontinuity; and so will the tendency of opportunistic conformists to get ahead, by scheming or approval, in unfamiliar contexts.

In fact, it may well be the case that the short-run effects of attempted transformation are greater than the longer-run effects. More can be done in upheaval than when life again acquires fixity. Kelley and Klein (1981) have argued precisely this point, on the basis of generalizing the case of the Bolivian Revolution of 1951.

Whether all this also entails the expectation that in the longer run incremental change will accomplish more than attempts at radical transformation we can perhaps leave an open question here. But note that the rulers of the Soviet Union came increasingly to view the achievement of cultural change as a matter for what they called "systematic social engineering" for—as Massell (1974) describes it—"a pragmatic commitment to relatively patient and systematic social action, wherein at least as much time and effort would be devoted to the building of bridges to traditional society . . . as to actual and direct confrontation with the traditional system."

Conclusion

It may well be the case that the political culture approach has been used to explain political changes in the sort of ad hoc and post hoc manner that saves—and thus weakens—theories rather than testing and strengthening them. Culturalists hardly have a monopoly on such theoretical legerdemain—certainly not when compared to rational choice theorists—when discomfiting facts confront them. But I have tried to show here that culturalists must have a strong propensity toward improvised theory saving when dealing with political change, since their assumptions lead, necessarily, to an expectation of cultural continuity—at any rate in a "pure" (abstract, ideal-typical) cultural world, where all matters falling under "ceteris paribus" are in fact "equal."

Nevertheless, it should be evident that a cogent, potentially powerful theory of political change can be derived from culturalist premises. The theory sketched here specifies that changes in dispositions, in response to contextual changes, should be pattern-maintaining changes or—if the contextual changes involve modernization—changes toward normative generality and flexibility; that in response to abrupt social discontinuities cultural dispositions should, for a considerable period, be "formless"—incoherent in individuals and fragmented in aggregates; that in such cases retreating into intact parochial structures occurs, while conformity should become ritualistic or opportunistic; that revolutionary artifice cannot accomplish cultural transformation in the short run; that such transformation will be attempted by despotic power or (mainly hopeful) legal prescriptions; and that, in the longer run, attempts at revolutionary transformation will tend to be regressive or at least have quite unintended outcomes. Note, however, that nothing here rules out engineered change, so to speak—attempted structural reforms of politics. In the modern world, political tinkering, on small or grand scales, is endemic. The theory simply states what should result from such tinkering.

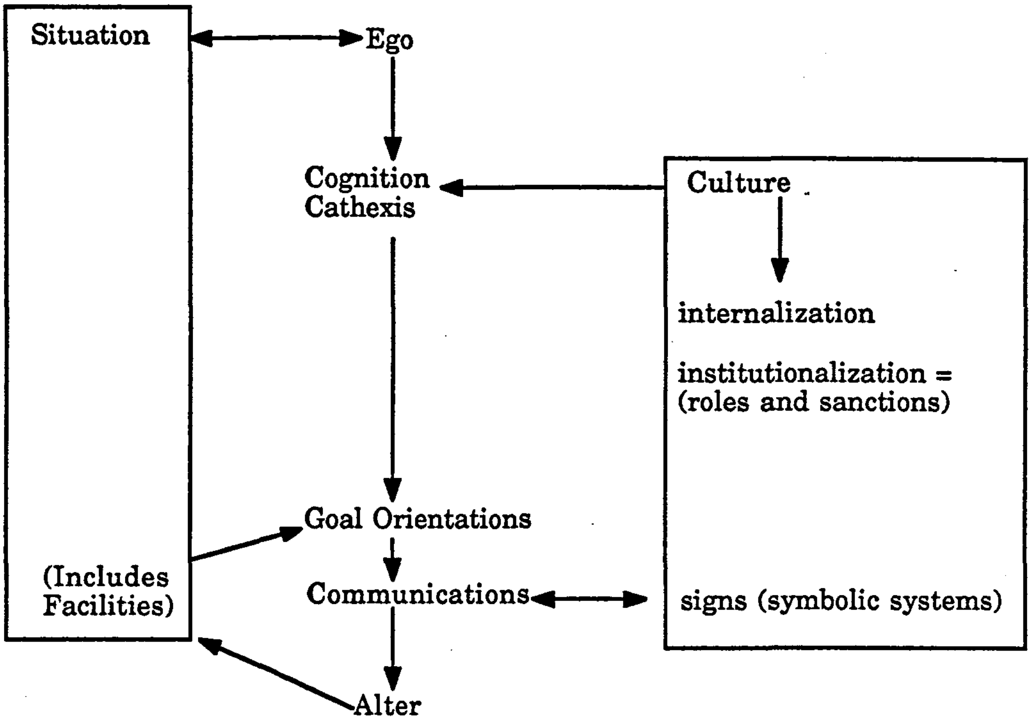
The problem of testing the theory against experience obviously remains, as do problems of operationalizing concepts for that purpose. But obviously theory comes first.

If the power of a culturalist account of political change is to be compared with that of different approaches to political theory and explanation, then general accounts of change, derived from noncultural postulates and similar to that presented here, are needed. Political-culture theories, admittedly, have not heretofore met the challenge of developing a general theory of change; but neither have others.

Appendix: *Culture*

The term *culture*, unfortunately, has no precise, settled technical meaning in the social sciences, despite its centrality in them. The variable and ambiguous use of key concepts generates unprofitable arguments that are merely definitional. Hence I append a note that places my use of the term, as sketched in the first section, in its conceptual context.

Figure A-1. Interaction in the Action Frame of Reference


Situation
Ego
Cognition
Cathexis
Culture
internalization
institutionalization =
(roles and sanctions)
Goal Orientations
(Includes
Facilities)
Communications
signs (symbolic systems)
Alter

My use of the term *culture* tries to make explicit, at the axiomatic level, what is implicit (occasionally almost explicit) in the works of Almond and his various collaborators (Coleman 1960; Powell 1966; Verba 1963; Verba 1979). Their use of the concept seems to be based squarely on Talcott Parsons' "action frame of reference." Parsons first worked out that "frame of reference" as a way of synthesizing four apparently diverse, all highly influential, early modern social scientists: Marshall, Pareto, Durkheim, and Weber (Parsons 1937). He and collaborators developed action theory in a large series of works, the most useful of which probably is the multiauthored book, *Toward a General Theory of Action* (Parsons and Shils 1951).

The action frame of reference is based, at the microlevel, on Parsons' notion of an interaction, societies being complexes of interactions (some earlier sociologists called them acts of "sociation"). The notion is depicted on Figure A-1. In brief translation, (1) ego (an actor) is in a "situation"—an objective context; (2) ego cognitively decodes that context and invests it with feeling (cathexis)—thus the context comes to have meaning for the actor; (3) the manner of investing situations with meaning is acquired through socialization, which consists mainly of early learning—this imparts the modes of understanding and valuing prevalent in societies or subsocieties or both. In aggregate, these may be called a society's "culture"; (4) socialization leads to the internalization of cognitive and affective meanings (viz., the cultural becomes personal) and their institutionalization (the definition of expected behavior in social roles and that of sanctions in case of deviation from expected behavior—these make smooth and regular patterns of interaction possible; (5) cognitions and affective responses to them define goals and ways to pursue them; (6) cognitions, feelings, and goals are communicated to alter (another actor) through the use of "signs" (symbolic expressions of culture that make ego's actions intelligible to alter)—but actions also depend on objective facilities that are part of any actor's situation and that independently affect the choice of goals; (7) alter responds, changing the situation in some respect, so that the process resumes.

Note especially that the action frame of reference emphasizes neither subjective nor objective factors but rather how the two are linked in interactions. Culturalists focus on the matters in the box on the right, but they should also bring that on the left into interpretation and theories. This I have tried to do throughout this essay, emphasizing how culture conditions change in varying contexts of objective change.

Alternatives to the notion of culture I use come chiefly from cultural anthropology. I use the plural intentionally, because the meanings of culture vary a great deal in that field. One can probably subsume these meanings under four categories: (1) culture is coterminous with society: it is the whole complex of the ways of a "folk," of human thought and action among particular people—Park (1937) comes close to that view; (2) culture is social life in its subjective aspects: the knowledge, beliefs, morals, laws, customs, habits of a society—one finds this meaning (and these illustrative words) in the seminal work of Tyler (1871) and, later, Benedict (1934) and Kluckhohn (1962); (3) culture is what differentiates societies from one another, for the purpose of idiographic description but also for theorizing through comparisons and contrasts (agreements and differences)—I take the seminal work here to be Malinowski's (1944); (4) culture is the distinctive, variable set of ways in which societies normatively regulate social behavior (Goodenough 1968; Sumner 1906).

The fourth set of meanings comes closest to that used here. My use of the concept of culture here seems to be justified by usage in political science and, more important, by its suitability to testing theories through the catholic deduction of unknowns once it postulates are explicitly stated. Anyway, my version of the concept is that about which theoretical conflicts have thus far occurred in political inquiry.

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THE DIALECTICAL LOGIC OF THUCYDIDES' MELIAN DIALOGUE

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If the realist tradition has underappreciated the formalizable quality of Thucydides' scientific investigations, neorealist teachers and writers have generally failed to see the normative and dramatical features of Thucydides' political science, each an expression of his dialectical epistemology and ontology. Nicholas Rescher's partial formalization of dialectics as a controversy-oriented approach to knowledge cumulation and Kenneth Burke's dramaturgical approach to textual understanding are both shown to fit Thucydides' argumentation in the Melian dialogue. Thus argumentation produces new knowledge about the inner determinants of Athenian imperialism; simultaneously it dramatically reveals the constituting practical rationale of Athenian actions to be unjust. Once Thucydides' determining essences of power politics are properly uncovered, their false "eternal, mathematical necessity" can be appropriately criticized. A case is thus suggested for a "neoclassical polimetrics" more fundamentally grounded in "political argumentation" about practical choices in particular contexts than in ahistorical laws, inductive statistics or deductive mathematics.

In writing his classic study of the Peloponnesian War, Thucydides sought "an exact knowledge of the past as an aid to the interpretation of the future, which in the course of human things must resemble if it does not reflect it" (1.22).[1] For twentieth-century realists, this has typically meant seeking timeless truths, or even mathematical laws, about the state's eternal, self-interested search for power, or the need to balance against it. Werner Jaeger's reference to "this political necessity, the mere mathematics of power politics" conveyed such a message to the German-speaking world of the 1930s. Indeed, in a book widely renowned for its penetrating account of Greek civic culture, Jaeger asserts that as evident in Sparta's fearful response to the growth of Athenian power, this "necessity" "is defined [by Thucydides] as the true cause . . . of the [Peloponnesian] war" (1976, 488; for similar views of other modern realists see Wight 1978, 24 and Morgenthau 1978, 8–9, 38).

Would not any other social scientist in our own century, if desirous of emulating the increasingly universal validity of the natural sciences and committed to building up reliable knowledge grounded in the deductive certainty of axiomatic formal argument and the "positive" evidence of our senses, also want to claim as their own Thucydides' search for "the clearest data," and his standard of objective precision: "the accuracy of the report being always tried by the most severe and detailed tests possible"? Would they not also want to "have written [such a] work, not as an essay which is to win the applause of the moment, but as a possession for all time" (1.23)? Influenced by apparently similar contemporary epistemological trends (developmentally described as "positivism," "logical positivism," "logical or analytical empiricism," and "critical rationalism" in Alker 1982), leading expositors of "scientific" neorealism focus their frequent, typically positive citations to Thucydides' classic on such instrumental, lawlike, causal analyses of state actions, possibly extended to include the structural determinants of these actions (Keohane 1983, 1986; Waltz 1979). Despite their objections to the imprecision, logical inconsistency, and empirical nontestability of too much traditional realism, contemporary neorealists still seek timeless laws of power politics, independent of moral praise or blame; they wish these laws to be objectively and falsifiably delineated with what Jaeger took to be mathematical precision.

Thucydides' work is equally well known today for the fatalistic, moral cynicism of the Athenian statement in the Melian dialogue (or conference) that "right, as the world goes, is only in question between equals in power, while the strong do what they can and the weak suffer what they must" (5.89). Associated with doubts concerning the possibility of deriving constructive, practical "lessons from history," one finds aspects of this critical tone in denials by structural neorealists that some policies or states have a more moral character than others (Waltz 1979, 127, 187n.). More historically accurate (but almost as bleak) is Keohane's appraisal that "classical Realism, with its philosophical roots in a tragic conception of the human condition, directs our attention in the twentieth century to the existential situation of modern humanity. . . . But Realism, whether classical or structural, has little to say about how to deal with that situation" (Keohane 1983, 519).

Surely Thucydides is one of the first[2] "scientific historians" (a phrase used in Jaeger 1976 and Strauss 1978, chap. 3; and supported by Keohane's arguments [1983, 506–10] for continuities in the "hard core" of the realists' "research program" from Thucydides to Waltz). But I think his epistemological conception of "scientific history" or "scientific politics" can more accurately and suggestively be described as a morally engaged version of dialectical logic, whose closest descendant is argumentation theory (Barth and Krabbe 1982; Eemeren, Grootendorst, and Kruiger 1987). His historiography combines a commitment to factual accuracy with what we would now call a dramaturgical perspective on human affairs (Harré and Secord 1972). Ignoring its dialectical origins, we seem almost to have forgotten that his scientific political history is at the same time one of the greatest morality plays ever written, in essence a Greek tragedy.

To better understand the relevance of Thucydides' insights and historical-political science for today's problems, I shall focus on the dialectical ways in which he presents, reconstructs and uses the Melian dialogue. I shall amplify the argument that Thucydides consciously used a sophistic dialectic in the writing of this dialogue (Finley 1939, 1942). He did this with a style and degree of formalizable rigor that compares favorably with more conventional contemporary modes of logical and statistical analysis. In contemporary language, his polimetrics sought to uncover, through critical, rational argumentation, the inner determinative structures, the criticizable and partly changeable constitutive practices of Greek politics.

The Melian Dialogue As Partially Formalizable Dialectics

Neither Popular Rhetoric nor Apodictic but Formal Debate

There are several levels of discussion in the dialogue, each with different epistemological import. The first level concerns the type of negotiation to take place between the Melians and the Athenians; it leads to agreement in favor of formal disputation.

The evidence for treating the Melian dialogue as a classical disputation or debate is explicit within Thucydides' text. Consistent with their oligarchic political character, initially derived from Sparta, their parent state, the Melians "did not invite [the Athenian] representatives to speak before the people [as would be the custom in a democratic state like Athens], but asked them to make the statement for which they had come in front of the governing body and the [oligarchic] few" (5.84). The Athenians put a good face on this choice by treating it as a preference for serious argument rather than for the set speeches of popular rhetoric by which the masses might be "led astray": they propose that the Melians "should instead interrupt us whenever we say something controversial and deal with that before going on to the next point" (5.85). The Athenians propose point by point philosophical debate about controversial matters rather than popular rhetoric.

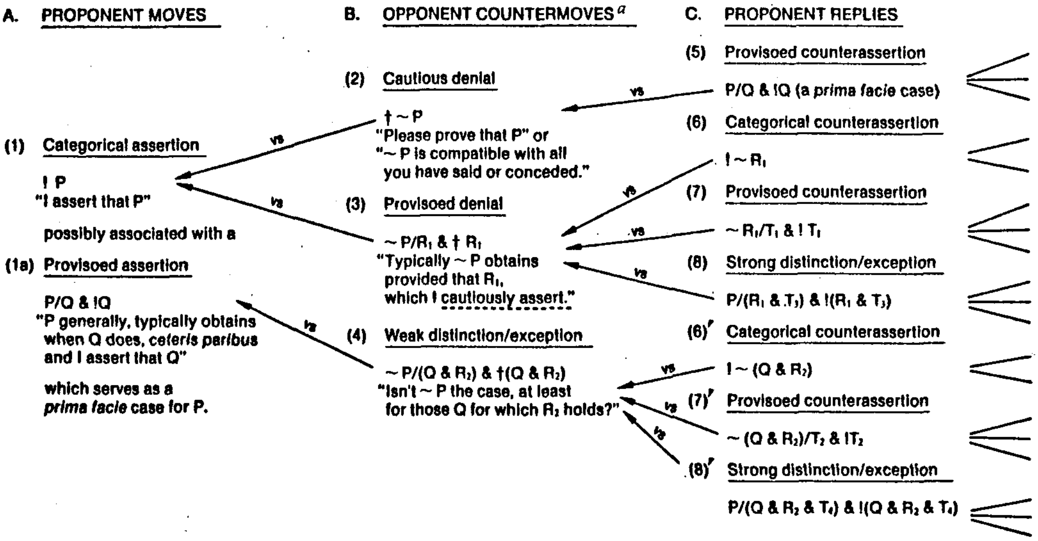
The kind of argument envisioned is also not what the Greeks (and Aristotle) would call apodictic (deductive, axiomatic, or necessary) reasoning. I suspect their model of such reasoning was geometric deduction, which had already achieved significant results by the end of the fifth century B.C. My evidence is the Melian statement that disputants "should be allowed to use and to profit by arguments that fall short of a mathematical accuracy" (5.90). Since their lives depend upon their persuasiveness, they want to make arguments that they can not strictly prove but against which effective counterarguments might not be made.

Both parties clearly have in mind some kind of uncoerced formal disputation (or "dia-logic," [Habermas 1971]) before a judge and an audience. To use Aristotle's terminology, we are in the realm of "dialectic," which he considered appropriate for political inquiry, the realm of arguable opinions about contingent or "probable" truths. Here dialectical inferences "must start from premises that command general assent rather than universal or necessary truths" (Aristotle 1964, 83, 153). Except for his somewhat more positive, democratic conception of rhetoric, Aristotle's *Rhetoric* and his *Topics*—together the first great Western treatise on dialectical argumentation—will assert the same distinctions several decades later (Arnhart 1981, chap. 2).

Coercive Irrationalities in the Dialogue

This agreement on the desirability of a dialectical mode of political discussion occurs in the realm of practical reason, where arguments start from reasonable assumptions, where strict deductions are often not appropriate, where better arguments still do not claim deductive certainty. But this realm is not completely "dia-logical"; nor does agreement extend to a second level of argument about the topic of discussion. The Athenian proposal only to discuss matters of self-interest is described without objection as "scarcely consistent with" the expectation of a reasonable exchange of views in "a calm atmosphere." The Melians see the Athenian invasion of their island as a "present threat" of making war against them, evidence that they have come "prepared to judge the argument [them]selves" (5.86). The Athenians do not deny this coercive element. A similar response occurs later (5.90), when the Melians note that the Athenians "force us to leave justice out of account and to confine ourselves to self-interest" right after the Athenians have asserted that for "practical people, the standard of justice depends on the equality of power to compel" (5.89). Evidently, Thucydides thinks like modern argumentation theorists who

Figure 1. Rescher's Simplified Formulation of Dialectical Move Sequences


A. PROPONENT MOVES
B. OPPONENT COUNTERMOVES[a]
C. PROPONENT REPLIES
(1) Categorical assertion
! P
"I assert that P"
possibly associated with a
(1a) Provisoed assertion
P/Q & !Q
"P generally, typically obtains when Q does, ceteris paribus and I assert that Q"
which serves as a prima facie case for P.
(2) Cautious denial
† ~ P
"Please prove that P" or "~ P is compatible with all you have said or conceded."
(3) Provisoed denial
~ P/R₁ & † R₁
"Typically ~ P obtains provided that R₁, which I cautiously assert."
(4) Weak distinction/exception
~ P/(Q & R₂) & †(Q & R₂)
"Isn't ~ P the case, at least for those Q for which R₂ holds?"
(5) Provisoed counterassertion
P/Q & !Q (a prima facie case)
(6) Categorical counterassertion
! ~ R₁
(7) Provisoed counterassertion
~ R₁/T₁ & ! T₁
(8) Strong distinction/exception
P/(R₁ & T₃) & !(R₁ & T₃)
(6)′ Categorical counterassertion
! ~ (Q & R₂)
(7)′ Provisoed counterassertion
~ (Q & R₂)/T₂ & !T₂
(8)′ Strong distinction/exception
P/(Q & R₂ & T₄) & !(Q & R₂ & T₄)
vs

Source: Rescher 1977, 6–15.

[a]It would also be possible to claim an entailment, such as $P \vdash S_1$ or (vis-à-vis move 1a) $Q \vdash S_2$), and then indirectly attack either P or Q with cautious or "provisoed" denials of S_1 or S_2.

would consider this conduct "irrational in [such a] dialectical situation."[3]

I next want to show a continuity between past and present epistemologies that has been neglected by neorealist writers: despite the coercive irrationalities, the third level of substantive discussion in the Melian debate, the debate proper, can be partially formalized and scientifically rationalized according to Nicholas Rescher's pragmatic version of dialectical inquiry (Rescher 1977; another application is Alker 1984). I shall emphasize the practical, truth-revealing contribution of this mode of scientific discourse. Except for her neglect of formalizability, Jacqueline de Romilly makes the same point about dialectical creativity in clarifying Thucydides' achievement: "Thanks to the dialogue form, the only one in the whole of the History, the particular and the general mingle together. This form enables maieutics to take the place of formal demonstration, so that the complete pattern Thucydides has in mind is revealed little by little, in all its aspects down to its deepest foundations" (1963, 274). The Greek root meaning of *maieutics* is particularly apt: "midwifery."

Rescher's Dialectical Logic

This demonstration requires first a summary of Rescher's formalization of the move and countermove possibilities within formal disputation, perhaps "the clearest, and surely historically the most prominent, instance of dialectical process."[4] This formalization is illustrated in Figure 1. Rescher recognizes that other, more or less formal characterizations of rational argumentation exist (e.g., Lorenzen and Lorenz 1978). But this simplified version is adequate for his primary purpose: to exhibit the "social nature of the ground rules of probative reasoning," the establishing of our factual knowledge through a social process of controversy-oriented rational argumentation.

Rescher's dialectical logic starts from, and fits, a social-legal scenario involving four unequal, truth-seeking roles: the proponent, the opponent, the judge or "determiner," and an audience. The proponent tries to establish a thesis, which the opponent attempts to discredit by insightful questions, denials or counterassertions directed against the proponent's assertions or their deductive implications. The proponent, like a legal claimant under Greco-Roman law, bears the burden of proof. Not only must he or she make an assertion, or "commitment," as part of every turn and avoid repetitions, but these assertions should nontautologically develop the argument with new grounds, distinctions, supportive theses or conditional reformulations. (Reformulations, such as moves 1a, 5, 8, and 8′ in Figure 1, are said to "discharge" the proponent's responsibility to defend a previous categorical assertion.)

The proponent's role, like his or her categorical assertions (symbolically preceded by exclamation points in the stylized tree of possibilities in Figure 1), is thus a "strong" one; the opponent's position is, relatively speaking, a "weak" one. Every unattacked assertion of the proponent becomes, at least temporarily, a "concession" by the opponent. Whenever the opponent makes a distinction as a way of limiting the scope of strong assertion, the stronger form of the assertion is also treated as a "concession." Although the opponent may develop counterarguments, he or she is not required to. The opponent's role requires only "weak" or "cautious" assertions (symbolically preceded by a dagger) at each turn. Characteristically, each cautious assertion may be reformulated as a question, that is to say, move 2 may be read as "Isn't the opposite of your thesis consistent with everything you have so far asserted or conceded?"

Rescher's determiner makes judgments on either formal or material criteria: the former include illicit circularities in reasoning, inconsistencies and incoherencies within one's belief system, and formal logical errors; the latter focus on the plausibility or implausibility of the proponent's "living," i.e. undischarged or unconceded, commitments. Appropriate evidential plausibility standards for participants and judges include source reliability and authoritativeness; the relative strengths of supportive evidence for rival theses; and the simplicity, regularity, uniformity (with other cases), and presumptive normality of such theses, where these features are seen as supportive of inductive systematization. A proponent must deepen the grounding and improve upon the reasoning supporting his or her original theses; an opponent must be judged for success or failure in finding weak links in such support.

The judge additionally oversees the conduct of the argument. A major initial simplification (presumably adequate for Rescher's purposes in the early stages of his book) is the assumption that erroneous evidential claims either are not made by the participants or are immediately corrected by the judge. Thus the determiner role has implicitly limited the move possibilities of Figure 1, where, for example, no circularities of reasoning are exhibited. This assumption has the important consequence for Rescher's model of formal disputation that disputing parties address themselves to matters of evidential substance rather than to the proprieties of formal or evidential reasoning. Formally, this will mean that the major dialogical connective of presumptive support, the (weak or strong) assertion of P "provisoed" upon grounds Q, P/Q, is never directly attacked in his examples. Indeed, when such questions are directly discussed in his later chapters, Rescher still does not provide a direct, formalized way of attacking P/Q. Only indirect challenges, involving the making of distinctions (or the allowing of exceptions) in the grounds supporting such assertions, are allowed. For example, although the strong exception move 8 of Figure 1, $P/(R_1 \& T_3) \& !(R_1 \& T_3)$, indirectly attacks the opponent's "provisoed" denial $\sim P/R_1$ in move 3, the focus formally is on the second part, the strong distinction $!(R_1 \& T_3)$, which "adds T_3" to the opponent's weak assertion $\dagger R_1$ in order to produce a strongly qualified reformulation, or "rewrite," of it.

Taking pains to subsume standard forms of sentential and syllogistic logic within his own approach,[5] Rescher nevertheless emphasizes the differences between his formalizations and the standard contemporary one. Thus $\sim$ symbolizes "negation" or "the denial of" the subsequent proposition, as it does in modern sentential logic, but for Rescher the weak assertion $\dagger\sim P$ suggests a need for substantive "refinement," not necessarily total rejection. As we have just seen, Rescher's *&* also sometimes involves a much more substantial combinatorial rewriting than the modern, purely formal logical *and.*

The "provisoed" or presumptive inference from Q to P, P/Q, is very different from either the $Q \supset P$ ("Q implies P") or $Q \vdash P$ ("Q entails P") of standard modern symbolic logic. Consider the way P/Q is read: "P normally, generally, typically, or as a rule follows from Q, ceteris paribus." Because this key inferential relation is only presumptive, it does not unconditionally support detachment of a conclusion Q from P & ($P \supset Q$). Hence, "in dialectical (as opposed to deductive) reasoning an assessment of the cognitive standing of a thesis never leaves its probative origins behind altogether." Alternatively, without detachment each dialectical argument requires inspection of the entire history of its derivation.

A Partial Formalization of the Melian Dialogue

As a test of the applicability of Rescherian formalizations, I shall offer and then discuss a formalization of the first major substantive debate in the dialogue, whether the submission of Melos to Athens, either voluntarily or through coercion, can be said to be in each of their interests. An italicized Rescherian formalization of each of the seven "moves" or "turns" in that debate will be placed under the corresponding Crawley translation text, with my interpretive insertions in that text indicated by square brackets. (I leave the formal analysis of the second part of the debate, concerning the prudence of Melos's hope for deliverance, to others.)

Move 1. Athenians. (a) We will now proceed to show you that we are come here [seeking Melian submission, voluntarily, if possible] in the interest of our empire, and that [what] we are going say [is in your interest, i.e., is] for the preservation of your country;

Let P_1 = Melian submission is in Athenian imperial interest and P_2 = voluntary Melian submission is in its (and Athens') interest.

$!P = !(P_1 \& P_2)$

(b) as we would fain [preferably] exercise that empire over you without trouble [through Melos's voluntary submission],

Let Q = imperially, Melos's voluntary submission is desireable.

$P_1/Q \& !Q$

(c) and see you preserved for the good of us both.

Let R = preservation of Melos via voluntary submission.

$P_2/R \& !R$

Move 2. Melians. And how, pray, could it turn out as good for us to serve [submit, become slaves] as [it is] for you to rule?

Let $P_2 = P_{21} \& P_{22}$, where P_{21} = Athenian interest in rule and P_{22} = Melian good from voluntary submission.

$\dagger(\sim P_{21} \& \sim P_{22})$

Move 3. Athenians. (a) [You would] avoid . . . suffering the worst;

Let W = Melos's avoiding the worst.

$$P_{22}/W \;\&\; !W$$

(b) [we would gain a more valuable tributary] by not destroying [you].

Let X = greater Athenian tributary gain.

$$P_{21}/X \;\&\; !X$$

Move 4. Melians. [Why could we not remain] neutral, friends instead of enemies, but allies of neither side?

Let N = Melos's friendly neutrality and I= Athens' interest.

$${\sim}P_1/(I \;\&\; N) \;\&\; \dagger(I \;\&\; N)$$

Move 5. Athenians. (a) Your hostility cannot hurt us,

$$!{\sim}(I \;\&\; N)$$

(b) as your friendship will be an argument to our subjects of our weakness,

Let S = subjects' regard for Athenian power.

$${\sim}I/({\sim}S/N) \;\&\; !({\sim}S/N)$$

(c) and your enmity of our power.

$$I/(S/{\sim}N) \;\&\; !(S/{\sim}N)$$

Move 6. Melians. Is that your subjects' idea of fair play—

Let $T = ({\sim}S/N)$ & $(S/{\sim}N)$, i.e., "that"

no distinction between people . . . unconnected with you and . . . your own colonists or else rebels whom you have conquered?

Let ${\sim}D$ = (making) no distinction.

$$T \vdash {\sim}D \;\&\; \dagger D$$

Move 7. Athenians. (a) [Yes! Our subjects] see no difference between right and wrong;

$$!{\sim}D$$

(b) those . . . still . . . independen[t] do so because they are strong, and if we fail to attack them it is because we are afraid.

Let N_1 = still being independent, ${\sim}S_1$ = their strength, and N_2 = not being attacked by Athens, and ${\sim}S_2$ = Athens' being afraid to attack.

$${\sim}D/(N_1/{\sim}S_1 \;\&\; N_2/{\sim}S_2) \;\&\; !(N_1/S_1 \;\&\; N_2/S_2)$$

(c) [So] that by conquering you we will increase [the] . . . size . . . [and] security of our empire. We rule the sea and you are islanders,

Let I_1 = increase in imperial size and security, I_2 = Athenian interest as sea ruler vis à vis islanders, and I_3 = particular Athenian interest vis à vis weaker islanders.

$$I \vdash I_2$$

(d) and weaker islanders too than the others; it is therefore particularly important that you should not escape.

$$P_1/(I_1 \;\&\; I_2 \;\&\; I_3) \;\&\; !(I_1 \;\&\; I_2 \;\&\; I_3)$$

The flow of Thucydides' argumentation evidences a truly remarkable fit with Rescher's rules of dialectical logic. As one kind of evidence, note the clear and repeated use of the specialized terminology of argumentation: *proceed to show; and how, pray; argument; distinction;* and *therefore* are terms of argumentative art that any broadly literate philosophical scholar will recognize. Secondly, there is the explicitly indicated "turn taking" mandated by Rescher; the more demanding requirement that previous arguments be met with new argumentation also holds. Although compound theses were not mentioned in Figure 1—they are an easy, natural and common complication of that formalism—the argument forms, from categorical assertions like (1a) to the "provisoed" counterassertions of (3a–b) and the weak distinction of move 4, clearly conform to Figure 1's evolving grammar of move, countermove, and reply possibilities.

Coincident with their major show of force, the Athenians are the proponent, they articulate the major theses of this discussion, and they offer an argument sketch for them. As opponents, the Melians regularly question, or weakly assert their views (2, 4, and 6). As I observed first hand in Uppsala at Lars Udehn's 1987 public doctoral thesis defense, the polite, cautious, carefully questioning, cooperative yet critical truth-seeking style of Melian interventions is still practiced in European universities 24 hundred years later! (Steven Lukes was the "opponent"; see also the neglected discussion of debates in Rapoport 1960.)

Further Reflections on This Correspondence

Going into the details of the argumentation formulae above opens up a deeper examination of some key issues raised by Rescher's proposal. Thus one finds there strong support for the centrality of ceteris paribus inference in (at least classical) political argumentation, thought of as a kind of "provisoed" assertion that subsumes standard formal inference and encourages novel, more adequate substantive reformulations. As for Rescher's subsumptive approach toward standard formal logic, it is harder to map Thucydides' approach into the modern sentential calculus. But there are at least several cases where roughly equivalent versions of the same proposition, e.g., P_1, P_2, I, S, N, T, and D (and their negations) are logically substituted for each other in a modern way. Moreover, an apparently analytic claim appears when the Athenian interest in controlling islands is somehow deduced without further evidence from its interest and internal nature as a power who "rules the seas" (7c). (Aron 1984 [p. 29] nicely spells out the requirements for maritime domination.)

Beyond including something like standard deductive inference within his informal but formalizable dialectical logic, Thucydides here seems to have encompassed important aspects of the "internal relations" of Athenian domination. Briefly, "internal relations," like Hegel's master-bondsman relationship, are those that (at least partially) constitute the identities (or essences) of the entities being related. Variously formalized in the Hegelian, Marxian, and analytical traditions, these identity-affecting logical relationships are usefully clarified in Bhaskar 1986, Elster 1978, and Ollman 1971. Since the argumentation shifts to another topic at this point, one is led to believe that at least the sketch of a deeper, provisionally plausible (and in effect conceded) explanation has been introduced.

Unlike standard formal logic, Thucydides' logic—his dialectic—is not tautological. Rather it is a rule and standard-governed, hence partially formalizable, practical epistemology. Within it there is always the possibility of new supporting or critical reformulations: both P_1 and P_2 are attacked and defended in a "provisoed" way, with (7d) being a very significant restatement of probative grounds for P_1, clearly more persuasive than those offered in the preliminary argument sketch.

In this light, the Melian attempt to reformulate Athenian imperial interest so as to make it consistent with Melian neutrality—the weak distinction of move 4—is an especially creative, revealing, as well as practically crucial, reformulation.[7] Not only does it make possible the knowledge enhancement due to the "internal relations" account of (7c), but practically it is a valiant, if ultimately inadequate and tragic, effort to deter Athens' attack.

Note that in the subsequent round the Athenians are put on the defensive, to the extent that they are induced explicitly to assert the denial of the Melian neutrality thesis. The argumentative requirement to support this new, negative claim produces (5b–c), which in turn allow the most revealing—and for a relatively neutral Greek determiner, probably decisive—move by the Melians: their inferential assertion of the Athenians' underlying warrant of no difference, no differentiation, no distinction in conduct expected from neutrals, colonists, and rebels, which at (7a) the Athenians clearly accept. For a society in which (according to Aristotle), justice is defined among educated citizens in terms of proportionality, or appropriate distinction, not only does this admission speak against the Athenians on this point, but it contradicts their own preference not to talk of the justice of their actions.

Rescher talks of dialectical negation as more than displacement, as "refinement." The formally correct law of double negation ($\sim\sim p = p$) is pragmatically abandoned, replaced by the possibility of an original thesis being *aufgehoben*, preserved in a refined form through a feedback "cycle of probative dialectic" that arises out of constructive, critical debate (1977, 67). His claim that his controversy-oriented dialectics produces cumulative, new knowledge, is either clearly or tentatively supported by each of the admissions (7a–d) of the seventh round of the Melian debate.

Rescher's preference for a dialectics of probative reasoning, one that does not challenge "provisoed" assertions per se, however, seems somewhat too restrictive. If one recognizes that P_1 might aptly be respecified as I/M, where M = (Melian) submission, one sees more clearly the intimate connections between the earliest and later stages of the argumentation. Indeed, the challenges to P_1 can all be said to challenge this implicit assertive connective. (See Devereux 1985 for the next steps here; reliable formalizations concerning contextually inferred elements are not, and may never be, easy to come by.)

Consonant with the possibility of deeper, more internally or characteristically revealing reformulations, a contemporary restatement of Thucydides' dialectical logic is as follows: Think (like Thucydides) of reality as layered and define scientific progress as the uncovering of the deeper, dialectically inner, determining layers or characteristic dispositions. (Systemic, political, economic, sociocultural, and ecological relationships may also play such inner, identity-affecting roles.) Recognizing that political systems are open and "autopoietic" (Bhaskar 1986), hence not apodictically decomposable, it is appropriate that investigative inferences are all in the classically tentative, ceteris paribus mode of reasoning rather than the ontologically flat statistical sense of ceteris paribus relationships going back to Ando and Fisher 1962 and Ando, Fisher, and Simon 1963. Because classical ceteris paribus reasoning corresponds closely to the "default reasoning" and "nonmonotonic inference" of contemporary artificial intelligence research on commonsense reasoning, which allow rationally defensible, nonadditive reconceptualizations, I am optimistic that Rescher's approach can be improved upon by more rigorous, internality-sensitive formalizations of dialectical, political reasoning (see Winograd 1980 and Reichman 1985).

Other Dialectical Elements in Thucydides' Historiography

We have now a better understanding of Thucydides' "logic," in the classical, dialectical sense of his (only partly formalizable) standards and procedures of valid argumentation and knowledge cumulation. In the one case where we have carefully examined its exercise, it has moved us more and more toward his ontology of things, his conception of the inner character of imperial systems, states, and statesmen. We have not yet clarified and explained the exceptional, dramatic, moral force of the Melian dialogue. Nor have we linked Thucydides' exceptional historical analysis of the Melian episode to the rest of his book. Each of these concerns also has a dialectical aspect.

Events Data and Speeches in Thucydides' Science

Let us begin with the way Thucydides interrelates events and speeches. Just as Finley observes (Thucydides 1951, xii), Thucydides' historiography "uniquely combined opposite tendencies of dialectic and observation, of generalization and observation." Throughout the book, speeches regularly come before the great actions (or "motions"). This sequencing is an explicit aspect of his interpretive, explanatory methodology: "My habit has been to make the speakers say what was in my opinion demanded of them by the various occasions, of course adhering as closely as possible to the general sense of what they really said" (1.21).

But this search for contextual appropriateness does not override the awesome sense of free, responsible, criticizable moral agency that this sequencing also conveys. Given the likelihood that many of the speeches were written or rewritten after the narrative was nearly finished (as Jaeger [1976, 342–47] and Romilly [1963, 275–86] argue), dialectically it is highly plausible to look for Thucydides' own critical interpretations of events, their motives, and consequences in the oppositions apparent in almost all of them. Indeed, the central role of the Melian massacre in postwar denunciations of Athenian imperialism suggests to Romilly a distinctive, quasi-legal significance for Thucydides' last revisions of the work, making it a "study [of] the policy of conquest as a whole, in relation to certain criticisms that had recently been made of it" (p. 286).

Does not Thucydides' account of Athens' response to the Mitylene revolt (a more serious affront than Melian neutrality, but occurring earlier in the war [3. 1–50]) contrast favorably with Athenian behavior at Melos? Cleon, "the most violent man at Athens, and at that time by far the most powerful with the commons" argued for the same standard as implemented in Melos—killing all male defenders and enslaving the others—with similar statements like "if, right or wrong, you determine to rule, you must carry out your principle and punish [all] the Mitylenians as your interest requires"; but Diodotus recommends "moderate chastisements," killing the oligarchic leaders of the revolt, not potentially pro-Athenian democratic masses, who had indeed turned against the Mitylenian oligarchs. Diodotus carries the day in a close vote with a final appeal that "good policy against an adversary is superior to the blind attacks of brute force" (3.49). Given Thucydides' stated preference for a much more limited form of democracy (8.97), Cleon's excesses may be seen as structurally linked to a commons that is too large.

Even more momentous comparisons critically link Athenian rationalizations in the Melian episode with Pericles' original statements of Athenian war aims and the immediately following Sicilian debate between Nicias and Alcibiades, the principal instigator of the fateful Sicilian invasion. Nicias argues against "risking things present for the sake of things future and uncertain," against the "madness of attacking a land which, if they prevail, they cannot hold," and against falling "sick of a fatal passion for what is beyond your reach" (6.1–13). Alcibiades, on the other hand, not long after having helped undermine Nicias's interim peace with Sparta by a spiteful trick (5.44), boasts that his "folly" (or "madness") brought benefits in alliances against Sparta and asserts that the Sicilian cities are unpatriotic, inhabited by "motley rabbles." He defends his extravagant sending of a record seven chariots to the Olympic games, scorns Nicias's do-nothing policy, calls for supporting their allies on distant Sicily, suggests that the initiation of a second front will "humble the pride of the Peloponnesians," and concludes that "a city not inactive by nature could not choose a quicker way to ruin" than inaction: "The safest rule of life is to take one's character and institutions for better and for worse, and to live up to them as closely as one can" (6.17–20). The interest of the multitude in the allies' exaggerated (and not disinterested) reports of great wealth in Sicilian temples and treasuries helps Alcibiades carry the day.

How clearly this debate, which repeats the theme of post-Periclean Athens' inherent aggressiveness, ironically replays the devastating strictures against groundless hopes in the Melian dialogue! How Alcibiades' remarks contrast unfavorably with Pericles' speech at the beginning of the war! Like Nicias much later, Pericles had been "more afraid of [Athens'] own blunders than of the enemy's devices," possessing many "reasons to hope for a favorable issue, if [the citizens] can consent not to combine schemes of fresh conquest with the conduct of the war" (1.143). On this contrast, Thucydides himself makes a rare explicit judgment, not in a speech, but in comments that constitute his encomium for Pericles at his death, 2½ years after the war began:

> The correctness of his previsions . . . became better known by his death. He told them to wait quietly, . . . to attempt no new conquests. . . . [Rather,] what they did was the very contrary, allowing private ambitions . . . to lead them into projects whose success would only conduce to the honor and advantage of private persons. [Moreover,] committing even the conduct of state affairs to the whims of the multitude . . . produced a host of blunders, and amongst them the Sicilian expedition; though this failed not so much through a miscalculation of the power [of the Sicilians] . . . as through a fault in the senders, . . . choosing rather [than continuing to help] . . . to occupy themselves with private cabals for the leadership of the commons, . . . [which] first introduced civil discord at home. (2.65–66)

The above extensive and rather exceptional quote confirms, then, the interpretation we have made on other grounds, that deliberative speeches like the Melian dialogue are to be compared for their implicit judgmental implications about the motives of the principal actors in Thucydides' history (see Mefford 1985).

Thucydides' Symbolic Oppositions and Principled Contradictions

Frequently, the debaters are leaders of different city-states or their contending factions; sometimes they are spokespersons for—or almost mythical symbols of —a frail but larger collective will politically composed out of contradictory inputs. Finley elaborates an oppositional theme we have already seen in the Melian dialogue's explanatory account (Thucydides 1951, xii–xiii): Athens is characterized as a naval power, based on an extensive commercial economy and political democracy (excluding women and slaves but typically including many of the militarily and economically necessary oarsmen); Sparta's nature is that of a land power, based on helot agriculture, controlled by a conservative oligarchy and a weak, rotating monarchy. Athens "encourages enterprise and initiative; Sparta emphasizes tenacity and tradition." These differences are discussed at length in the introductory sections (5.84–85) and the later parts (5.97, 99, 107–11) of the Melian conference.

Strauss puts some of these oppositions (and others) into a partial hierarchy of dialectical distinctions: "Just as humanity divides itself into Greeks and barbarians, Greekness in its turn has two poles, Sparta and Athens. The fundamental opposition of motion and rest returns on the level of Greekness; Sparta cherishes rest whereas Athens cherished motion" (1978, 157). Even more remarkable (and extreme) is his argument, "[In] the Peloponnesian War . . . one sees Greeks at their peak in motion; one sees the beginning of the descent. The peak of Greekness is the peak of humanity. The Peloponnesian War and what it implies exhausts the possibilities of man. . . . All human life moves between the poles of war and peace, and between the poles of barbarism and Greekness. By studying the Peloponnesian War Thucydides grasps the limits, . . . the nature of all human things. It is for this reason that his work is a possession for all times (1978, 157).

Despite their ethnocentric excess, consonant with our Rescherian analysis, these truly remarkable quotations point to the deepest answers Thucydides gave to the quest signaled by his words quoted in my opening paragraph. Raymond

Aron has made rather similar points: "Politics is dialectic when it unfolds between men who mutually acknowledge each other. It is war when it brings into opposition men who . . . wish to remain strangers to one another. . . . By the same token, we perceive why war is the completion of politics and at the same time its negation" (1984, 23). The common thought is at once dialectical in its revelatory search for fundamental, substantial oppositions and transformations and hermeneutical in its reconstructive suggestions of a grammar of motives, and of opposed organizing principles spanning or generating the space of possible human political activity (see Alker 1982 and Habermas 1973).

Thucydides' Greek Tragedy

At last we are ready for the crucial second thesis in this reconstruction of the Melian dialogue: Thucydides' scientific study of power politics is essentially dramatic and ultimately tragic. Because the failures of both the Melians (a Spartan colony) and their opponents, the Athenians, are revealed in that dialogue, a larger Greek tragedy is suggested by it.

The first argument is a textual one. At the same time that it is a turning point in Thucydides' entire "scientific history," the Melian dialogue is pure drama (cf. Aron 1984, 28). To quote Cornford, at the point in the Melian conference where the dialogue begins, "the historian changes from narrative to full dramatic form, prefixing as in a play, the names—'Athenians,' 'Melians'—to the speeches" (Cornford 1971, 174–87). The dialogue was in fact a principal feature of Greek tragedy of the period as practiced by Euripides and others. By Thucydides' shifting into this form, Cornford argues, the dramatic ironies of this pivotal turning point are highlighted. These concern both the blind hubris, Eros, insolence, and pre-Christian hope infecting both the Athenians and the Melians.

The second argument for the dramatic nature of Thucydides' Melian dialogue is more substantive, with essentially the same tragic conclusion. It begins by noting how Thucydides' use of dialectical substance, "the overall category of dramatism," conforms to the dramatist's search for the roots of human action in verbal terms, for instance, symbolic oppositions, and principled contradictions. Strauss and Aron have already shown us how, "when men are treated in terms of other things, men may even be said to speak for the dumb objects [or forces] of nature" (Burke 1969, 33).

Burke then offers a directly relevant, dialectical conception of tragedy (and science):

> Galileo speaks of experimental testing as an "ordeal." Stated broadly, the dialectical (agonistic) approach to knowledge is through the act of assertion, whereby one suffers or calls forth as counter-assertions the kind of knowledge that is the reciprocal of his act. This is the process embodied in tragedy, where the agent's action involves a corresponding passion, and from the sufferance of the passion [by the original agent or the empathetic observer] there arises an understanding of the act, an understanding that transcends the act. In this final state of tragic vision, intrinsic and extrinsic motivations are merged. . . . Although purely circumstantial factors participate, . . . they bring about a *representative* kind of anecdote . . . that belongs with the agent's particular kind of character. (1969, 33)

As a Burkean "representative anecdote," the Melian episode reveals the blindness of "tyrannical Eros," of blind Athenian hubris and its insolent defiance of the gods. (Cf. Burke 1969; Cornford 1971; and Strauss 1978, 225–27; Alker [1987] suggests formally reconstructing Thucydidies' narrative in terms of the fundamental tragic notion of a "mythical actor's self-annihilation.")

Burke even claims that "we can . . . catch glimpses of a relation between dialectics and [Platonic] mathematics . . . in the fact that *mathemata* means both things learned . . . and the mathematical sciences" and that "we can discern something of the 'tragic' grammar behind the Greek proverb's way of saying 'one learns by experience'; '*ta patemata mathemata,*' the suffered is the learned" (1969, 39–40). As Strauss says, the Peloponnesian "War surpassed the Persian War in regard to human suffering," caused (intrinsically) by men and (extrinsically) by nature (1978, 150).

It turns out, then, that the Melian dialogue is a key scene in a classical morality play about might and right, not simply an eternal statement of the mathematical truths of realpolitik. An increasingly blind, arrogant, lustful, imperious Athens will pay for its failings with the lives of many of its citizens and, eventually, with its independence as well. Like the most thoughtful modern realists, Thucydides joins normative, descriptive and explanatory investigations pointed toward the suffering-based "learning" of moral lessons.

On the Appropriation of the Classics

In a recent, postmodernist treatise, *On Diplomacy,* James Der Derian has attempted "to explain a[n international political] system by studying the genesis of its internal relations, which are seen as expressions of alienated *powers.*" A student of a leading classicist of our era, Hedley Bull, Der Derian, too, cites the authority of Aron and Thucydides as support for his own approach, quoting a passage pointing ironically to subsequent Athenian injustice: the Corcyrans say to Athenian and Corinthian representatives on the eve of the outbreak of the Peloponnesian War, "Every colony, if it is treated properly, honours its mother city, and only becomes *estranged* when it has been treated badly. Colonists are not sent abroad to be slaves of those who remain behind, but to be their equals" (1987, 2, 6; Der Derian's emphasis). As equals, they could exhibit their practical, moral capacities for arriving at reasonable, just bases for concerted political action in times of need. His book thus suggests that certain aspects of a neoclassical orientation can legitimately be attributed to the present effort as well.

Neoclassical Polimetrics

I have objected to the view of mathematical realpolitik derived from Thucydides' work and attributed to him by many of Thucydides' realist and neorealist interpreters. By showing how much of his analysis they (and deductive behavioral modelers like Zinnes [1975]) have lost, I have tried to motivate and to legitimate a more comprehensive form of rigorous, partly formal, political analysis. The dialectical methodology of Thucydides' classic work has been available in principle since the dawn of scientific history, yet the above formal analysis of the Melian dialogue still seems, to this author, full of as yet unrealized potential.

For one trained in the powers of formal analysis, that exercise suggests what a neoclassical polimetrics of political activities and constitutional structures would look like (cf. Alker 1975). It would be a polimetrics dialectically grounded in human experience, focused on contextually appropriate practical-normative standards of just conduct and institutional worth. Whether addressing empirical topics or arguing policy alternatives, its preferred "logic" would be argumentation theory, using classical ceteris paribus reasoning. Quasi-experimental inference, conventional mathematical probability and statistics, and deductive modeling would be seen as special cases or components of this more general approach. (Dunn [1982] anticipates many of these arguments; Campbell [1982] concedes most of them.)

We have seen how political argumentation, if grounded in a cooperative, uncoerced, truth-seeking orientation and used skillfully to ask the right questions—those that critically probe the essential justifications—can suggest key determinants and consequences of sociopolitical identities, actions, relations, institutions, or contexts. Polimetrics built up from a foundation in political argumentation can help reveal the underlying, contestibly legitimate political orders (constitutions), the principled practices and constitutive relationships that, if known, will make possible further, even more penetrating, policy-relevant discussions of a critical and reconstructive sort. Its cumulative results are unlikely to be timeless laws; its universals are more likely to reflect hegemonic or pluralistic political achievements.

Der Derian's genealogical approach is justified as an effort to refute the existence of any ahistorical, timeless, defining essence of international politics. To study discursively, genealogically, historically the internal relations of power politics is to deny its eternal reality. My own rereading of Thucydides' Melian dialogue suggests a similar, open-ended, dialectical epistemology of practical knowledge cumulation, ideally to be combined with competing sets of hypotheses concerning the inner and outer determinants of Athenian (and Spartan) actions. It thus locates itself where any epistemologically self-conscious contemporary approach to international politics ought to be—in the midst of contending claims about lessons of the past and possibilities for the future.

Thus we too may participate in the engaged, dramatic, but relatively objective and rigorous kind of historical political analysis that Thucydides was engaged in. Like the Athenians and their Greek friends before the war, we can scientifically appeal to dialectical judgements, relatively unconstrained argument and counterargument, "impartial laws . . . [and] differences settled by arbitration" (1.76–77). We can join different sides of the inner contradictions of "Greeks" and "barbarians," enlisting Thucydides' (or Euripides' even more antimilitaristic) insights in our own political-scholarly debates. By dialectically engaging ourselves in the endless, passionate search for objectively accurate and motivationally superior historical accounts, we critically renew the past and help create the future.

Notes

Since the 1980 draft of this paper, supported by NSF grant number 7806707 to the Center for International Studies at MIT, a John T. and Catherine D. MacArthur Foundation grant to that center and to the Swedish Collegium for Advanced Studies in the Social Sciences have furthered its refinement. Among the many commentators on earlier drafts, I am most indebted to Karl W. Deutsch, Erik Devereux, Roger Karapin, and F. J. Tickner. Dwain Mefford proposed I try to formalize Thucydides' dialogues.

1. The customary form of citation is to the books and paragraphs of the Greek text. Except where a secondary source is cited, my quotations are taken from the Crawley translation (Thucydides 1951), now in the public domain.

2. Jaeger (1976, 483) argues that Hecataeus first took "the scientific and rational approach to the facts of human life as the essence of history," while Herodotous gets the credit for introducing "the religious and dramatic element" into history. A more globally valid view should take into account Chinese Confucianism, Legalism, and Taoism (Freiberg 1977).

3. Barth and Krabbe (1982, 58) associate with the opponent's role "an *unconditional* right to criticize" proponent's assertions, and reconstruct the pragmatic rules of logical argumentation to include rule FD E5super: "When party N performs a speech act that is not among those permitted to N by the rules of this system of formal dialectics, or if it performs a non-permitted, nonverbal action that reduces the other party's chances of winning the discussion, then N has lost all its rights in the discussion and N's behavior is to be called *irrational with respect to the present dialectical situation* by the company that has adopted this system of formal$_3$ dialectics" (p. 63).

4. My partial summary of Rescher's (1977) argument, including quotations, are taken primarily from chapter 1, pages 1–34; see also chapters 2 and 5.

5. Indeed, Rescher suggests that his formal dialec-

tics goes nicely with Aristotle's formal syllogistic reasoning, modified to include an "all-things-being-equal" operator (1977, 13–14). He follows Aristotle on this point, who argues that syllogistic principles apply both to axiomatic demonstration and to dialectic (Aristotle 1964, 153–59).

An even more ambitious synthesizing move between the dialectical and standard, that is, formal and deductive, logical traditions is due to Paul Lorenzen, who defines the standard logical constants —*&*, v, ~, *if . . . then*—dialectically (i.e., argumentationally), in terms of strip rules for their use in "critical dialogues" between proponents and opponents. "Such a 'social' definition of the logical constants may be regarded as a theoretical elaboration of Wittgenstein's notion of a *language game.*" It suggests a new "dialogical or argumentational 'garb' " for "modern propositional and predicate logics" shown by Paul Lorenzen to be demonstrably equivalent to them. Lorenzen equates "classical logic with a logic of cooperative debates (dialectics, in Plato's and Aristotle's sense), and [twentieth-century] intuitionistic (constructive) logic with a logic of competitive debates (eristics)" (Barth and Krabbe 1982, 12, 24, 55). The set of 21 definitions and 40-odd rules proposed for bringing both Cartesian monological axiomatics and post-Hegelian associative thinking into the discursive, socially and rationally arguable realm is given in chapter 3 of the Barth and Krabbe text, from which one rule was cited in note 3. The relevant original papers are collected in Lorenzen and Lorenz 1978. Further integrations and elaborations, of direct relevance to political science, are clearly needed.

6. Apparently, Sergeev's recent, rich, philosophically informed formalization of Thucydides' argumentation omits the discussion of internal relations (Sergeev 1986). His published paper cites the European literature of note 3 and the 1980 version of the present paper.

7. "Recognition of the central role of distinctions in the dialectical enterprise—based on the division (*dihairesis*) of key concepts—goes back at least to the Socrates of Plato's *Phaedrus* and is doubtless present in the theory and practice of the early Sophists" (Rescher 1977, 12, n. 13). Later (pp. 66–67), Rescher's description of "constructive negation" exactly fits the creativity of Melian distinction making: "When P/Q is succeeded by ~P/(Q & R), there is not just the displacement from P to ~P, but also the refinement (amplification, improvement) from Q to (Q & R) . . . : it advances the discussion and shifts the issue onto a more sophisticated ground."

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CAN LIBERALS PUNISH?

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Liberalism doubts that a sound theory of politics can be built from a theory of the human good. In pursuit of its authorized ends, the liberal state can establish incentives and disincentives, it can require reparations, and it can restrain dangerous persons. But can liberals punish? By distinguishing punishment from related phenomena and comparing its presuppositions with central tenets of liberalism, I tentatively conclude that they cannot. An analysis of efforts by leading liberal theorists to come to terms with punishment confirms that suspicion: their theories do not authorize punishment or do so at the expense of their more fundamental principles. I conclude that the ability to punish criminals is part of a larger moral experience and capacity that is lost with liberalism's expulsion of the human good from politics, and further, that without reference to the human good, liberalism's central concepts of liberty and equality are left weak and empty.

Liberalism is a doubting philosophy of politics. It doubts that the human good, or human excellence, exists; or if it does exist, it doubts that we can know it; or if it does exist and we can know it, liberalism doubts that we can ever know it well enough or gain sufficient consensus about it to provide a stable base for political life. As the founders of liberalism knew only too well from the religious wars, when the right way to live becomes the focus of politics, differences of opinion may yield to conflicts of arms, and anger can be spurred to fanaticism. Thus liberalism builds its theory of politics as distant as possible from a theory of the human good. Though liberalism is doubting, it is not apparently disabled—certainly not, it would seem, in practice. Under its title has grown the most powerful and prosperous nation in the world's history. Where it has prevailed, science and technology have flourished, politics have been stable, and life in general has been healthy, peaceful, and comfortable.

Nor does its doubt seem disabling to its theory of politics. Indeed this very doubt about a human good emboldens its claim to reveal a dignified and stable base for political life.[1] This confidence expresses itself in a variety of forms: Locke's natural rights of life, liberty, and estate; utilitarianism's greatest happiness of the greatest number; Kantian and—more significantly today—neo-Kantian moral rights of equal concern and respect. To be sure, there is disagreement among these theories: for Locke and his progeny, the state is confined largely to the task of securing physical protection of the individual and establishing rules for the acquisition, protection, and transfer of property; while for neo-Kantians and utilitarians the state may, even at some expense to Lockean rights of property, pursue ends expressing an aggregation of the people's "personal preferences."[2] But

AMERICAN POLITICAL SCIENCE REVIEW
VOL. 82 NO. 3 SEPTEMBER 1988

more striking is the agreement;[3] and this consists in an affirmation of the principles of liberty and equality, the importance of the individual over the community, and the idea of the neutral state, that is, the idea that the state should set its policies as far as possible from any particular notion of the human good. On this neo-Kantians and Lockeans agree. For Ronald Dworkin, the fundamental neo-Kantian right to equal concern and respect means that government "must not distribute goods or opportunities unequally on the ground that some citizens are entitled to more because they are worthy of more concern. It must not constrain liberty on the ground that one citizen's conception of the good life . . . is nobler or superior to another's" (Dworkin, 1978, 272–73). Similarly for his fellow neo-Kantian, Bruce Ackerman, "Neutrality [always capitalized in his work] forbids citizens from justifying their rights by asserting the possession of an insight into the moral universe intrinsically superior to that of their fellows" and "forbids the legal recognition of any right that requires its holders to justify its possession by declaring themselves intrinsically superior to their fellow citizens" (Ackerman 1984, 99).

And for the natural rights tradition of Locke, as articulated by Walter Berns, free and equal people form a covenant where "each man agrees that his opinions of good and bad, right and wrong, justice and injustice, are just that—private opinions—and the principle of equality requires him to acknowledge that other opinions, even if they are contrary to his own, have as much (or as little) dignity as his own" (1982, 60). Since judgments about the human good are regarded as merely personal and inherently arbitrary,[4] laws, especially the fundamental law, must be "neutral."

So, in pursuit of its authorized ends, the liberal state can enact and enforce laws; it can make people whole whose rights have been trespassed; it can restrain or incapacitate dangerous persons; it can establish incentives and disincentives for engaging in certain activities. But can liberals punish? Can they punish without overreaching the bounds of the individual upon whom they set their state, without appealing to notions of a human good that they set apart from their state, or without vindicating the anger that their peaceable form of politics seeks to tame?

The Meaning of Punishment

To answer this question we must consider what punishment is, as distinguished from similar phenomena, such as taxes, reparations, and penalties. Bright-line distinctions are not to be expected, for the phenomena blur and blend at the margins; yet their central and distinguishing features can be made out rather clearly. Taxes (or licenses), like punishments and penalties, may discourage activities, but they do not usually prohibit them. Licenses or taxes in effect say, "If you wish to engage in conduct A, then you must pay fee X or take exam Y"; punishments and penalties say conduct A is not to be done. Of course, the distinction might blur. Taxes may be prohibitive and the penalties may be trivial, so in reality the real messages are reversed. But nominally and usually, penalties and punishments are easily distinguished from taxes.

Penalties, of course, lie closer to punishment than do taxes but are still distinguishable. While penalties say that some conduct is not to be done, typically, as for example with speeding tickets for marginally exceeding 55 mph, that message is given with less seriousness than in the case of punishments; the hardship imposed, the shame attached, the rigor with which we are held to the mark, and the vigor with which violators are pursued all are less. And not uncommonly the penalty is accompanied with other implicit messages that bushion the impact of the penalty, assuring you that though you broke the law, you are really not a bad person; that anyone could have done this sort of thing, that it could have happened to anyone, or that the penalty is a risk you run for engaging in a certain line of conduct and for public benefit you have to be held to the mark. As suggested by these messages, the lawmaker, in framing penalties, will often consider strict liability a reasonable option; whether you did the action or not becomes the only question, not whether you were justified or whether you had an excuse. But where punishments are involved, justification and excuse are essentially considerations, and strict liability is virtually unthinkable.

The availability of strict liability for penalties points to its essential difference from punishment in objective and focus. The focus of a penalty is the future and its principal object is to deter. This can be done better if would-be violators know that they can't slip through the system by faking an excuse or justification. Because the offender may have had good reason or excuse, strict liability must occasionally be unfair. But exactly because the offense in question is not thought "serious" in the sense of reflecting deeply on the character of the offender, the loss in fairness is balanced by the gain in deterrence.

If the primary focus of a penalty is the future (deterring future offenses), the primary focus of a punishment is the past (what was done and why it was done). In looking back, punishment resembles reparation. But reparation looks back in order to make the victim whole, and it does this typically as a matter of private law; reparation is not essentially a concern of the public. Punishment, if it does make the victim whole does so metaphorically and as an incident of its essential public concern. In sum, punishment appears distinct from taxes, licenses, penalties, reparation, and methods of deterrence; apart from its similarities, there is something left over, something that cannot be made up by extracting elements from the other phenomena similar to it.

Perhaps the most useful access to the phenomenon has been given in Joel Feinberg's identification of the essence of punishment with its public, authoritative expressive function, its "expression of attitudes of resentment and indignation, and of judgments of disapproval and reprobation" (Feinberg 1970, 98). Though its two elements, attitudes of indignation and judgments of reprobation, function together, the expressive function can be clarified if we consider them separately.

Attitudes of resentment and indignation direct anger toward the person *responsible* for the crime. If the person has a complete excuse (e.g., good faith mistake or total accident) or a justification (e.g., self-defense), anger is inappropriate. Only if the person is responsible do we feel anger and seek revenge. As Walter Berns has said, "We can become angry with an inanimate object (the door we run into and kick in return) only by foolishly attributing responsibility to it, and we cannot do that for long, which is why we do not think of returning later to revenge ourselves on the door" (1979, 153).

Anger, according to Aristotle, is "an impulse, accompanied by pain, to a conspicuous revenge for a conspicuous slight directed without justification towards what concerns oneself or towards what concerns one's friends." It is also, he noted, accompanied by a "certain pleasure" arising from the anticipation of revenge (1954, 1378). While anger may be for a conspicuous slight to oneself, in a narrow and personal sense, the anger that underlies punishment at law is for the wrong done more broadly to "what concerns oneself" or one's "friends." This anger displays a caring for persons and matters beyond one's narrow self-interest and expresses the desire to vindicate them. It does not fix to the harm a person or a principle suffers, at least not directly, but to the cause of that harm in the action of a responsible human being and the attitude displayed in that action for the things we care about. ("Contempt, spite, or insolence" are the three forms of slighting mentioned by Aristotle.) Punishment expresses and satisfies that longing for vindication.

As James Fitzjames Stephen wrote, the criminal law gives "shape to moral indignation, and hatred of the criminal. . . . The sentence of the law is to the moral sentiment of the public what a seal is to hot wax" (1883, 81). The criminal sanction gives proper shape or form to anger, measuring it according to what Feinberg identifies as the second element expressed by punishment, the "judgments of disapproval and reprobation." That judgment is essentially one of desert, that the deed deserves hard treatment, shame, and reprobation in proportion to the gravity of the crime. This stern, measured judgment makes punishment what it is. "One may lose more money on the stock market than in the courtroom; a prisoner of war camp may well provide a harsher environment than a state prison; death on the field of battle has the same physical characteristics as death by sentence of law" (Gardner 1953, 193). It is the condemnatory judgment alone, expressing the ideas of desert and righteous vindication, that renders "mere" hardship a punishment.

But what is meant by the term *desert* in this context? As William Galston has noted "every desert-claim is of the form: *A* deserves *x* in virtue of *f*" (1980, 170). Here *x*, as suggested above, is the hard treatment embodying the vindictive resentment and reprobation of the political community. But the *f*, the nature of the crime, and the relation of *x* to it need to be explored further to understand the "fitness" implied in the word *desert*. Recalling Aristotle's definition of anger, we see that it is directed to a "slighting"—an act destructive of what we care about, with an attitude of insolence, contempt, or spite. What is crucial is not the destruction alone but the attitude, or state of mind, with which it is done. A killing may be an occasion of regret but not anger if it is totally accidental; it may even be the occasion for celebration under certain conditions. Only if the killing displays the wrong attitude toward life does it occasion the desire for revenge and a sense of "fitness" of punishment.

It is proper then to see punishment and blame as the opposite of rewards and praise.[5] We praise and reward people to express our appreciation of their deeds and our admiration of the beauty of character their deeds bespeak, elevating, displaying, preserving, rendering more secure the things we care about. We punish and blame people to express our resentment and disapproval of their deeds and our detestation of the ugliness of character their crime bespeaks. At both ends of the spectrum we intend the deserved treatment to penetrate deep to the self, to mark our judgments of the person in virtue of the deed. In this way rewards differ from compensation and incentives; and punishments differ from reparation and disincentives. Compensation and incentives, reparations and disincentives all do not, and are intended not to, reflect deeply on the person; they are largely external to the self. Though liberalism has gone far to diminish the range of the self that can be publicly recognized, for most citizens the self appears more expansive and they wish to mark its dimensions; they wish to erect monuments of gratitude to reward the greatest of heroes with immortality through fame (Hamilton, Madison, and Jay 1961, 432, 437) and to punish the vilest of criminals with death. Punishment thus invokes a vision of the moral order where the virtuous flourish and the wicked suffer (cf. Feinberg 1970, 217; Ross 1930, 56–65).[6]

Why Liberals Can't Punish: Prima Facie Considerations

My purpose has been not so much to *justify* punishment as to describe the phenomenon and thereby indicate the sort of justification it requires. But if this description is accurate, we can readily see why punishment should trouble liberals. First, punishment expresses and satisfies righteous anger; liberalism, as witness to the horrors of religious warfare, seeks to eliminate as far as possible the role of anger in political life and refuses to countenance its expression. Liberalism does this by taking out of political life the things men and women love most and thus would be most likely to become angry about—God, country, virtue—and focusing instead on the relatively tame concerns of individual, material well-being (see, e.g., Berns 1979, 150–51; Franklin 1964, 149–50; Montesquieu 1949, bk 20, chap. 1). Punishment transcends these concerns, for we punish not just to discourage future wrongs to our well-being but to express our indignation and reprobation at the wrong that has been done.[7]

Second, punishment concerns the public, the community;[8] liberalism builds from the individual. Punishment vindicates the community in its standard of right, not the individual in his or her personal concerns or even personal rights. Liberalism has no trouble explaining reparation and restraint (see, e.g., Locke 1952, 2.8, 11), for these do build from material concerns and rights of the individual; but punishment outstretches these.

Third, punishment inflicts pain on the criminal distinct from what is necessary for deterrence or rehabilitation, and it does this in virtue of the character displayed in the criminal act; it thus requires what Dworkin has called an "external preference." Liberalism opposes these (Dworkin 1978, 275; Hart 1962, 65). As Mill stated liberalism's "one very simple principle," "the sole end for which mankind are warranted, individually or collectively, in interfering with the liberty of action of any of their number is self-protection. That the only purpose for which power can be rightfully exercised over any member of a civilized community, against his will, is to prevent harm to others" (Mill 1959, 13). More recently, Feinberg has persuasively extended the liberal case for "the moral limits of the criminal law" from Mill's "harm principle" to what he calls the "offense principle" (Feinberg 1985). But whether harm or offense to the individual marks the outer limits of liberalism's use of the criminal sanction, that authority cannot reach beyond the hard treatment necessary for reparation and restraint; it cannot warrant the distinct pain resulting from the "external preference" expressive of reprobation and resentment.[9]

Fourth, and closely related to the preceding points, the pain that punishment delivers to the criminal bears a statement about the human good and excellence and this individual's relation to it; policy based on such visions and desert in virtue thereof are precisely what liberalism seeks to avoid.[10] In fact it is this point that undergirds the others; for it is this vision of human virtue and vice that fuels the anger, that sustains community judgment, and that vindicates the pain visited upon the criminal through punishment.

How Do Liberals (Try To) "Punish"?

If I am correct so far, punishment is fundamental to our social experience, but the principles of liberalism disable it from punishing. My characterizations of liberalism, however, might underestimate its variety and resourcefulness. Thus it is useful to consider the distinct accounts of punishment offered by leading liberal thinkers to determine first whether liberals do in fact authorize punishment (as distinct from "penalties") and second if they do, whether it is not with serious damage to other principles that they affirm.

Although Locke uses the words *retribute* and *punishment,* he makes no pretense that his theory of politics would authorize punishment as I have described it. Natural rights authorize one to "retribute to [a criminal], so far as calm reason and conscience dictate, what is proportionate to his transgression, *which is* so much as may serve for *reparation* and *restraint;* for these are the *only reasons* why one man may lawfully do harm to another, which is that we call punishment" (Locke 1952, 2.8, my emphasis). Not surprisingly those who have followed in his footsteps do not authorize punishment either. Richard Posner, the leader of the law and economics movement, finds an adequate explanation of all fundamental concepts of criminal justice, whether ancient (1981, 207–27) or modern (1985, 1193–1231), in terms of whether they are "functionally efficient," given the society's stage of development, towards the larger goal of "wealth maximization." The neo-Lockean Robert Nozick, while placing significantly greater emphasis than Posner on individual rights, similarly avoids real punishment. For him the criminal law properly begins only at the point where the mechanisms of private, tort law prove inadequate for making individuals whole. This is the point where there is "uncompensable fear"[11]—where reparation is rendered impracticable by the nature of the wrong (such as violent assault) or the number of people involved (the countless third parties suffering fear of the assault, especially if they know there will be no greater penalty than medical costs and lost time compensation). But the ends of private law—reparation and restraint—remain the same; criminal law is simply a continuation of the same by other means (1974, 54–78).[12]

In marked contrast to Locke, neo-Lockeans, and utilitarians, Kant eschews any essential concern with deterrence or any other consequence of punishment and embraces instead a purely retributive account; he thus challenges the thesis that liberals cannot punish. Vindication of the moral law must be the focus and "woe to him," he warns, who rummages through the serpentine paths of utility looking for advantages and disadvantages of punishment (Kant 1965, 100). For Kant, retributive punishment follows from his categorical imperative as an immediate corollary. As a criminal you act directly contrary to a principle you could deem universal law, and your measure of punishment is directly proportional to your distance from that categorical imperative. "Any undeserved evil that you inflict on someone else among the people is one that you do to yourself. If you vilify him, you vilify yourself; if you steal from him, you steal from yourself; if you kill him, you kill yourself" (Kant 1965, 101). By inflicting on criminals evils proportionate to what they have done to their victims, the state vindicates the universal moral law.

One might object that Kant has misapplied his own categorical imperative. Instead of saying, of a murder for instance, "If you kill him, you kill yourself" and that capital punishment vindicates the moral law, one might ask, purporting greater faithfulness to the categorical imperative, "Could I affirm capital punishment as universal law, knowing that it could be applied to me?" That was the general line of argument pursued by the criminologist Cesare Beccaria in his argument against capital punishment (1963);

and as we shall see, it is the course taken by contemporary neo-Kantians. For Kant, however, this objection confuses the ideal self, the *homo noumenon* of the categorical imperative, with the ordinary self, the *homo phenomenon* of experience; it confuses the personality of the lawgiver with that of the subject. The latter should have no voice in the enactment of the law, for as subject of the law, one would have interests that would corrupt what Kant calls the "holy" character of legislation, transforming the categorical imperative to a merely hypothetical one. Thus in determining the moral content of the law as well as the punishment of the violations of it, one must rigorously adhere to the perspective of the holy legislator, not the interested subject (Kant 1965, 105).

If this sharp distinction between the self of concrete experience and the ideal self distances Kant from contemporary liberals and seemingly bolsters the claim that at least this liberal can punish, it also points to a more fundamental deficiency in Kant's liberalism. The ideal self is so abstracted from human experience, having no interest, desire, longing, or inclination whatsoever to corrupt the purity of its will—having no "nature"—that it seems no longer human. With no knowledge of what is good for humankind, or what constitutes human happiness, the will of the ideal self may be "free," but it also has no direction; thus any action, as long as it allows for self-consistency, meets the criterion of the categorical imperative (Sidgwick 1890, 210).

More radically stated, a purely free will is both senseless and worthless. Your will is free only if it flows from what is truly you; it is not free if it results from something alien to you. Thus prior to freedom, as Bergmann has observed (1977, 37), is the question of identity; without identity, freedom has no sense. Without identity, freedom is also worthless, for we value freedom only because, by virtue of our identity, we have purpose and make distinctions of rank among our purposes (Taylor 1979, 183).

By expunging from it every interest and inclination, Kant sought to establish and maintain the purity of the ideal will (Kant 1959, 5, 10, 24, 29, 31). But he also drained it of its human identity and thus of its ability to identify a crime; that is, to know an action as a crime, we must be able to identify at least something of what is good for humans. Consider Kant's statement quoted above: "Any undeserved evil that you inflict on someone else among the people is one that you do to yourself. If you vilify him, you vilify yourself; if you steal from him, you steal from yourself; if you kill him, you kill yourself." To gather Kant's meaning, we must import the knowledge that he excludes from the categorical imperative. For only if we know that reputation, property, and life are human goods can we say that vilification, stealing, and murder *are* evil. Without such knowledge, these actions might be thought blessings that should be honored rather than punished; without such knowledge, we cannot even distinguish honor and punishment. Far from accounting for punishment, Kant cannot even account for crime.

For sound reason, then, unadulterated Kantianism is less influential today than neo-Kantianism. The latter sacrifices the purity of the categorical imperative in order to endow its subject with human identity and human purposes; at the same time it seeks to preserve Kant's universalism by defining moral law as that which a person would enact, knowing that he or she might occupy any place in society, have any interest, and hold any traits compatible with human identity. On the question of liberty, then, neo-Kantians agree with Kant, holding that each adult is entitled to as much liberty as is compatible with equal liberty for all. Of those thinkers that we can describe roughly as neo-Kantians, H. L. A. Hart has given the most sustained effort to come to terms with the phenomenon of punishment.

In his 1962 lectures, *Law, Liberty, and Morality,* Hart noted the tension between punishment as "emphatic denunciation" and liberal principles limiting the criminal sanction to the prevention of harm to others. Emphatic denunciation as a justification for punishment "represents as a value to be pursued at the cost of human suffering the bare expression of moral condemnation, and treats the infliction of suffering as a uniquely appropriate or 'emphatic' mode of expression" (p. 65). For the retributionist, the suffering of the criminal is good; for Hart it is an evil, justifiable only if it produces less harm and suffering for others. Yet in the series of articles published as *Punishment and Responsibility,* he also noted that a simple focus on preventing harm would support indefinite sentences, widespread preventive detention, and the expansion of strict liability, eliminating excuses and justifications (1968, 28–53, 159–85). In other words, the simple goals of preventing harm, if unrestricted, would threaten liberty and sacrifice fairness.

Hart's solution was to distinguish between (1) the "general justifying aim" of the criminal law (preventing harm) and (2) the "distribution" of the criminal sanction, that is, determining who is liable to the criminal sanction and to what degree. The general justifying aim would render us all vulnerable to the criminal sanction; we may be the hapless victim of circumstance whose exemplary suffering at the hands of the state will secure others from harm. But impinging on this general aim is what Nozick would call a "side-constraint" in the distribution of the sanction. Only when a person has chosen inexcusably and without justification to commit the offense in question (and is in this sense responsible for it), is there "title" to impose the sanction (1968, 4). Our innocence thus protects us from what otherwise would be our vulnerability to the criminal sanction, allowing Hart to maintain that liberty and fairness are the real substance of this side-constraint; for it functions to maximize "within the framework of coercive criminal law the efficacy of the individual's informed and considered choice in determining the future and also his power to predict the future" (1968, 46; see also 44–48, 180–85).

The point is neatly made and seems at first to accord with our moral sense. Yet there are two problems with Hart's account. First, if "efficacy of the individual's informed and considered choice" is our goal, not just "within the framework of coercive criminal law" but in life generally, it is not clear that we should limit the criminal sanction to those who have chosen to commit a crime. After all, reflective choices in life can be, and probably are today, more often thwarted by unpredictable criminal actions than by unpredictable criminal sanctions. Depending on the social circumstances, we might maximize our general ability to effect informed and considered choices by lessening our individual control over the "distribution" of the criminal sanction—say by eliminating excuses or allowing an occasional innocent to be sacrificed for the appearance of swift and certain sanction—in order to gain a greater security against crime. A 5% increase in the risk of imprisonment may be offset by a 50% decrease in the risk of being robbed, raped, and murdered.

Second, even if Hart's "distribution" principle would work as he intends and not as I have described, he really would not be "punishing" the criminal. In terms of its forward thrust or positive justification, the sanction is applied to you as a criminal not because you have committed a crime, but because your suffering would be useful. The positive justification remains the same whether you commit the crime or not. The difference is that the protective side constraint of innocence is withdrawn. If a jury were to speak honestly of its Hartian judgment, it would not pronounce its conviction of the criminal as *guilty* but as *not innocent*.[13]

Probably no liberal thinker has given more thorough development or more rigor of analysis than John Rawls to the basic neo-Kantian project of endowing the subject of justice with a human identity, of correcting the impersonality of Kant's categorical imperative without at the same time sacrificing its impartiality. His account of punishment thus merits our attention. The main idea of Rawls's theory of justice as fairness is sufficiently familiar that I need only note a few central points. The basic principles of justice as fairness are said to be those that free, equal, and rational actors would adopt in the "original position." In that position, we abstract ourselves from everything alleged to be morally arbitrary—the distribution of genetic endowments, positions in society, conceptions of the good, psychological propensities—and choose behind a "veil of ignorance" the principles of justice for the society we are to inhabit, knowing only that we will have some conception of the good and some combination of other morally arbitrary features without knowing which will be ours. The subject of justice is thus given a human personality and the categorical imperative is rendered hypothetical, but impartiality and universality are preserved. In such a position, according to Rawls, the rational actor would affirm two fundamental principles: first, "Each person is to have an equal right to the most extensive basic liberty compatible with a similar liberty for others" and second, "Social and economic inequalities are to be arranged so that they are both a) to the greatest benefit of the least advantaged [the so-called 'difference principle'] and b) attached to offices and positions open to all under conditions of fair equality of opportunity" (Rawls 1971, 60, 83).[14]

For the most part, Rawls assumes "strict compliance" with the principles of justice; that is, his is an "ideal theory" in which there is no injustice, whereas punishment for him belongs to "partial compliance theory," or how to deal with injustice (1971, 8, 315). Though his two discussions of punishment then are brief and incidental to the elaboration of his two principles of justice, each is nonetheless revealing. In the first account, his larger topic is "the rule of law," and his concern is to show how central precepts of this concept follow from the first principle of justice, equal liberty. Even with rules deemed fair from the perspective of the original position, if there are no authoritative sanctions, instabilities to social cooperation will result. If you cannot assume that others will abide by the terms of the social contract or other implicit and explicit agreements, your own fidelity might set you at a disadvantage. Thus to give effect to the principles you would choose in the original position as well as later implicit and explicit agreements, you would affirm the use of sanctions against those who fail to abide by their agreements. And to prevent those sanctions from unduly restricting your liberty, you would as a general principle limit their application to instances when you have chosen, without excuse or justification, to break a rule. Thus as in Hart's argument, which Rawls closely and explicitly tracks, punishment is understood as an extension of liberty: "I have maintained that the principles justifying these sanctions can be derived from the principle of liberty. The ideal conception shows in this case anyway how the nonideal scheme is to be set up; and this confirms the conjecture that it is ideal theory which is fundamental. We also see that the principle of responsibility is *not founded on the idea that punishment is primarily retributive or denunciatory*. Instead it is acknowledged for the sake of liberty itself" (1971, 241, my emphasis; see also 576).

Hart's explanation obviously fits with

Rawls's theory of justice as fairness. Indeed Rawls may be said to supply the larger justifying framework that Hart merely implies. But Hart also drew a conclusion that Rawls merely implies: "punishments" in the neo-Kantian account become indistinguishable from "tariffs": "The pains of punishment will for each individual represent the *price* of some satisfaction obtained from breach of law" (1968, 47, my emphasis). Such a statement, Hart acknowledges, "can sound like a very cold, if not immoral attitude toward the criminal law, general obedience to which we regard as an essential part of a decent social order"; but we feel the sting of this cold immorality only because we have in mind criminal laws of "whose operation we approve." If instead we think of the criminal laws of "South Africa, Nazi Germany, Soviet Russia," he tells us, the account looks much better (1968, 47). That certainly is true. But for an account of punishment purporting to derive from a theory of justice and an extension of liberty, it is a poor recommendation that it becomes palatable only if set in the context of tyranny.

In Rawls's second account of punishment, he defends his "difference principle" (that inequalities should be distributed in a way that supplies the most benefit to the least advantaged) against the contrary notion that "income and wealth, and the good things in life generally, should be distributed according to moral desert" (1971, 310). This latter principle is strikingly similar to that which we previously noted as attached to true punishment—the virtuous should flourish and the wicked suffer. And as Rawls admits, it is a supposition of common sense. But "justice as fairness," he tells us, "rejects this conception," for it would not be chosen in the original position. There one would affirm differences in the distribution of shares not in order to recognize desert but to encourage people to do things socially useful. Since we abstracted from everything that could serve as a basis for desert when we placed ourselves in the original position, it is hardly surprising that justice as fairness would have nothing to do with moral desert.[15] Even a person's conscientious effort cannot serve as a basis of desert, for it too is largely determined by "natural abilities and skills and the alternatives open to him," that is, by things that are morally arbitrary (1971, 312).[16]

However well these points allow Rawls to distance his theory from the commonsense view, they do not really refute that view; and this fact poses a special challenge to his theory of justice, for the "original position" in itself is not his final test of morality. Instead, the original position obtains moral force only as it brings our conceptions of the principles of justice into "reflective equilibrium" with our "considered convictions" of what is just in a wide range of applications (1971, ix, 20). To achieve that equilibrium we are permitted to adjust our considered convictions as well as our understanding of the principles of justice (and thus the characterization of the original position). All is tentative as we move back and forth adjusting principles and convictions until equilibrium results. Since the commonsense view of distribution is obviously a widespread considered conviction, it would seem that Rawls must either (1) show that upon mature reflection we will find the commonsense view unacceptable or (2) adjust the principles of justice and the conditions of the original position to accord with common sense. In fact, Rawls does neither of these. Instead he maintains that the commonsense view derives from an entirely distinct sector of the moral universe, that of retributive justice. The account of punishment that ensues is much as I have described it: desert-based, retributive, and responsive to the marks of "bad character" displayed in the criminal act.

Dividing the moral universe might allow a semblance of equilibrium within this part of Rawls's analysis, but it also reveals a teetering disequilibrium between his two accounts of punishment. In the first account, as we noted, Rawls agrees with Hart that punishment is *not primarily retributive* but is instead a scheme of "penalties that stabilize a scheme of cooperation." Here, in this second account, he maintains that punishment is *essentially retributive* and "*not* simply a scheme of taxes and burdens designed to put a *price* on certain forms of conduct and in this way to guide men's conduct for mutual advantage" (Rawls 1971, 314–15, my emphasis). In the first account he says that "the ideal conception [of strict compliance] shows . . . how the nonideal scheme [of partial compliance] is to be set up; and this confirms the conjecture that it is ideal theory that is fundamental." Here he maintains that "criminal justice belongs for the most part to partial compliance theory, whereas the account of distributive shares belongs to strict compliance theory and so to the consideration of the ideal scheme. To think of distributive and retributive justice as converses of one another is completely misleading" (1971, 315). In the first account, the original position maintains its claim to give a comprehensive account of justice, but allows only for penalties. In the second account, the original position accounts only for a portion of justice and cannot itself account for punishment.

The retreat of claims for the original position in the second account may have been strategic; but it is hardly convincing, for the moral universe cannot be so neatly divided. If you deserve punishment by virtue of the bad character you display in a criminal action, as Rawls in his second account suggests (1971, 315), it is hard to see why you do not deserve rewards for the good character you display in an act beneficial to society (cf. Sandel 1982, 89–91). And if these points are true, the just moral world should in some way resemble that where the virtuous flourish and the vicious suffer—and not simply that where all inequalities work to the benefit of the least advantaged.

The Pathology of Liberalism

Though liberals can deter, they cannot punish. But if they acknowledge deterrence as their primary goal, they cannot effectively deter either. A simple cost-benefit analysis—the risk of being caught and punished against the gains of crime—should convince most that crime pays and pays well. We abstain from crime—at least most of us still do—not simply because it might be disadvantageous but because it is wrong. The criminal sanction must express that conviction. It cannot simply say that crime is against your interest; through retribution and emphatic denunciation, it must also express, reinforce, and sustain the sentiment that crime is wrong (Durkheim 1984, 31–67; Goodhart 1953, 92–93; Wilson and Herrnstein 1985, 493–98). As political institutions in liberal societies are more self-consciously explained and justified through liberal theory, the question thus becomes more pressing whether liberalism can defend itself. This practical question invites the consideration of whether liberalism suffers a deep theoretical defect.

As a doubting theory, liberalism expresses itself in negative terms; its banner ideals of liberty and equality are pressed against things. Liberty is pressed against arbitrary restrictions; equality is pressed against false claims of superiority—noble birth, race, gender, social status. In the name of these ideals, enormous energy and capacity has spilled forth to create the wonders of Western liberal democracies. But when it is finished rejecting false claims, what is left that liberalism affirms? Depending on their starting points, liberals seem inclined to admit that one or the other of their ideals is empty.

Thus Dworkin announces, upon reflection, "It seems to me absurd to suppose that men and women have any general right to liberty at all, at least as liberty has traditionally been conceived by its champions" (1978, 267). An astonishing statement, but as Dworkin develops the point, it seems correct. For all laws do in some way diminish liberty—restricting a citizen "from driving his car uptown on Lexington Avenue," for example—and we see no need to justify such laws by any greater claim than that the general interest is thereby served. Where we do see liberty trumping the general interest in the strong sense of a "right" to liberty—for instance, permitting a citizen to speak "his mind on political issues"—there is something *behind* the claim to liberty and it is that something rather than liberty itself to which we have a right.[17] For Dworkin, drawing deeply upon Rawls's *Theory of Justice*, it is equality, or more precisely equal concern and respect, that must fill the empty concept of liberty (1978, 272).

But through a meticulous analysis, Peter Westen (1982a) reached the conclusion that *equality* is the empty term. All claims of equality take the form that some persons or things are equal in virtue of some trait; and because of that trait they should be treated the same. The moral force of such claims then rests entirely with the relation of the trait to the treatment; once we make that claim, there is no more work for equality to do. All claims of equality are really invitations to look for a trait in virtue of which the persons or things are equal; it is the traits that really count, not the claim of equality. Again, a superficially astonishing, but deeply persuasive statement.[18] In fact Rawls—whose theory led Dworkin to conclude that liberty rested on equality (Dworkin 1978, 150-83)—would seem not to disagree, at least not in his more recent work, for there he derives equality from certain attributes of liberty. People are *free* "in virtue of what we may call their moral powers," that is, "a capacity for a sense of justice and a capacity for a conception of the good." People are *equal* "in virtue of having these powers to the requisite degree to be fully cooperating members of society" (Rawls 1985, 233). In other words, contrary to Dworkin's understanding of Rawls, Rawls himself (at least as of 1985) holds that liberty does the work; equality simply affirms that one has those traits to the requisite extent. Dworkin, however, must be forgiven for thinking that equality is more fundamental; for in *Theory of Justice* Rawls had placed precisely these same traits under the heading of equality, not liberty (1971, 19, 505-12). The point, however, seems clear: liberty and equality are themselves empty. When liberalism is pressed for its substance, it has to look elsewhere, to a conception of human nature or the essential self,[19] which for the neo-Kantians seems largely determined by the idea that the citizen of the just state has a "capacity for a conception of the good."

Even here, what promises substance turns hollow upon scrutiny. For there is no intimation of any truth beyond mere conceptions of the good, no essence to which they point; conceptions are not to be ranked in value (Rawls 1971, 19). Human happiness remains to be invented ex nihilo by each for him- or herself (cf. Nietzsche 1954, 128-31). Ackerman states the case most dramatically: "But can we *know* anything about the good? . . . The hard truth is this: There is no moral meaning in the bowels of the universe" (1980, 368). This point is thought to support liberty, because if we know nothing about the good or if the good does not exist, then all restrictions on our liberty are arbitrary. And it is thought to support equality, because it claims to superiority by virtue of good character or superior knowledge of the good are groundless. But in fact the point is poised on the brink of nihilism; for if there is no good or knowledge of it, then there is no reason for me *not* to impose my will on yours. *And the only meaning of equality is that* we are equally worthless (Galston 1982; Sandel 1982).

With atrocity as well as nobility of character and deed both emerging from things "arbitrary from a moral point of view" (Rawls 1971, 15), the hero will get a pension but not a reward, and the villain will get a penalty but not a punishment; both will get what they are "entitled" to and neither will get what he deserves, for "the notion of desert seems not to apply to these cases" (Rawls 1971, 104). Whether you are to become a hero or villain is shrouded by the veil of ignorance. The thought recalls a comment by Nietzsche: "There is a point in the history of society when it becomes so pathologically soft and tender that among other things it sides even with those who harm it, criminals, and does this quite seriously and honestly. Punishing somehow seems unfair to it, and it is certain that imagining 'punishment' and 'being supposed to punish' hurts it, arouses fear in it. 'Is it not enough to render him *undangerous?* Why still punish? Punishing itself is terrible'" (Nietzsche 1966, sec. 201).

In light of this latent nihilism, one might be tempted to conclude that liberalism has done well only because it has been able to feed its empty ideals on the corpse of ancient virtue. But this conclusion, while apt regarding liberal theory, is unfair to liberal practice; for in practice virtue has never been allowed to die. Liberal polities remain capable of punishing their criminals, of expressing righteous anger and reprobative judgment; and conversely they honor their heroes with office, fame, and glory. Yet as they have grown more self-conscious in the defense of their institutions and looked simply to liberal theory for their sustenance, liberal polities have grown in self-doubt, as certainly they must if they heed the advice of leading liberal theorists.

At its inception, liberalism was properly horrified by religious warfare, torturous treatment of criminals, the cramping of the human spirit, and more—all done in the name of some notion of human excellence. It was prudent then to lower the horizons of political concern. But when lowering becomes leveling, new problems arise, not just in the tedium of the new moral landscape, the incompetence of the political vocabulary to meet the range of our moral experience, and the emptiness of its theoretical account but also in the practical inability of liberalism to defend itself against those who would violate its laws.

Notes

1. For the most part, classical and contemporary liberals recognize unequivocally this link between skepticism regarding the human good and confidence regarding their affirmative principles. Dworkin denies the link, but that denial seems clearly to result from his confusion of *good* and *right*. For instance, in his essay, "Liberalism," Dworkin maintains that liberalism cannot rest on "skepticism about theories of the good," for "its constitutive morality provides that people must be treated as equals by their government, not because there is no right, but because that is what is right" (1985, 203). In other words he fails to understand that the charge of skepticism claims not that liberalism has no idea of what is right but that this idea of right is itself derived from skepticism about the human good (Brubaker 1985). He repeats the error in his most recent work (1986, 441).

I cannot consider here all varieties of liberalism. I do not, for instance, consider recent endeavors of thinkers such as William Galston (1980) or Rogers Smith (1985) to place a theory of the human good within liberal theory; nor do I consider older efforts, such as John Stuart Mill's, to distinguish between higher and lower pleasures (1959, 76; 1969, 10:255). To date, such efforts have had little impact on the mainstream of contemporary liberalism, largely I believe because they run contrary to its central animating spirit. I am concerned with Locke, neo-Lockeans, Kant, and especially the neo-Kantians. These compose the mainstream of Anglo-American liberalism, extend a broad front across the "left-right" continuum, and on key points, as will be suggested below, present a striking agreement.

2. This, as opposed to external preferences, or preferences for goods or opportunities for another. This distinction is drawn from Dworkin (1978, 275).

Like all attempts to state the line between individual rights and public authority, this distinction proves problematic upon analysis. See, for instance, Dworkin's discussion of whether people who adore "Sarah" may have their preference counted that Sarah's preferences should count for twice as much as those of anyone else (1985, 361–72). I use it merely to suggest the general direction in which the neo-Kantians wish to establish the authority of, and limits to, public action.

3. Excepting cruder forms of utilitarianism, which I shall leave aside in this discussion.

4. "The mind has a different relish, as well as the Palate; and you will as fruitlessly endeavor to delight all men with Riches or Glory . . . as you would satisfy all Men's Hunger with Cheese or Lobsters. . . . Hence it was, I think, that the Philosophers of old did in vain enquire, whether Summum Bonum consisted in Riches or bodily Delights, or Virtue, or Contemplation. And they might have reasonably disputed, whether the best Relish were to be found in Apples, Plumbs, or Nuts; and have divided themselves into Sects upon it" (Locke 1979, 2.21.55, p. 269).

5. This is not to claim that there is a perfect symmetry; for, as Aristotle notes, "to inflict a just penalty or punishment is indeed an act of goodness; but it is also an act which is forced on the agent, and it has value only as being a necessity. Acts done with a view to bestowing honours and wealth on others are in a different category: they are acts of the highest value" (1958, 1332a).

6. For some, the tie to this ethical view undermines the case for punishment as I have described it; for it is thought absurd, and correctly so, that we could in determining our sentences decide on "the right amount of suffering to inflict in a given case [by assessing] the character of the offender as manifested throughout his whole life and also his total lifelong balance of pleasure and pain" (Feinberg 1970, 116–17). But when we reward and punish, these "whole life" concerns enter our judgments only at the margins. We hesitate to honor a battle hero if, but for this single shining moment, he or she was an insolent and selfish coward; we are reluctant to slap a lengthy sentence on the bribed public official if, but for this solitary lapse, he or she was a dutiful, honest, and faithful public servant. But the essential focus is on the deed, the character that is displayed in performance of that deed, villainous or heroic, and the treatment deserved in virtue of that deed.

7. In cases of outrageous crime, to prevent the natural anger that liberalism refuses to countenance from swelling into a rage that would threaten the stability of the polity and hence the rights of individuals, liberals might find it expedient to punish. But that is not to say that they could then consider themselves justified in any positive sense, only that they were trying to prevent the greater evil of threats to individual rights.

8. Cf. Durkheim 1984, 63: "[Punishment's] real function is to maintain inviolate the cohesion of society by sustaining the common consciousness in all its vigor."

9. One way of extending the authority of liberalism to enable it to punish would be through the concept of "profound offense"; but realizing its threat to the foundations of liberalism, Feinberg explicitly rejects it as a basis for criminal sanction (1985, 50–96).

10. Adhering to its commitments to individualism and against external preferences, liberalism might in a paradoxical manner still attempt to account for community and desert and from such an account to justify punishment; that is, liberals may be said to constitute a community in their common commitment to individualism or to have a notion of desert in their valuing of the inner disposition not to judge individuals on the basis of their inner dispositions. In a similar sense, one may say that a relativist is really an absolutist because he or she believes relativism to be the truth. But the concepts thus formed of community, desert, or absolutism are understood only in reference to the principles that contradict them; they cannot form the foundation of anything that assumes genuine community, desert, or absolutism. To their credit, the liberal thinkers considered below largely avoid this paradoxical morass.

11. Nozick's focus on "fear" as the exclusive emotion of concern is itself revealing. Directing its force inward upon the individual self, fear forms the emotional base for the enactment of penalties and prohibitive taxes, but it cannot explain punishment. Anger, directing its force outward to the source of harm, forms the emotional base for punishments.

12. In a later work, Nozick does define punishment in terms that are largely compatible with the description I have given, but he makes no significant attempt to link this description with his justifying principles (1981, 363–97).

13. As will be pointed out below, such an account of the criminal sanction renders "punishment" a mere "tariff." It should be noted that in the *Concept of Law*, Hart persuasively argues, from the "internal perspective," for the distinction between punishments and taxes (1961, 39). But as Neil MacCormick observes in his intellectual biography of Hart, when it comes to punishment, Hart is not consistent in his "hermeneutic method" (1981, 143). The transformation of *guilty* to *not innocent* is also accomplished by Norval Morris (1974, 75) and Herbert Packer (1968, 67–69). Ely encounters similar difficulties (1980, 135–79; Brubaker 1982, 228–31).

14. I quote Rawls's first statement of these principles since it is clearer than his more technical and precise second formulation (1971, 302).

15. On the need for a *basis* for desert, see Feinberg 1970, 55–94.

16. Note also Rawls's comments on the man of "superior character": "his character depends in large

part upon fortunate family and social circumstances for which he can claim no credit" (1971, 104).

17. "If we have a right to basic liberties . . . because an assault on basic liberties injures us or demeans us in some way that goes beyond its impact on [a general right to] liberty, then what we have a right to is not liberty at all, but to the values or interests or standing that this particular constraint defeats" (Dworkin 1978, 271).

18. Though obviously it is not persuasive to everyone. See, for instance, the articles by Burton (1982), Chemerinsky (1983), and Greenawalt (1983) and the replies to each by Westen (1982b, 1983a, and 1983b).

19. In his more recent work, Rawls professes that his theory of justice is "political not metaphysical." But I do not see how he can avoid making at least implicit "metaphysical" claims either about human nature or history. Either he rests his argument on certain ideas of the nature of man or on a moment in the course of human history. It seems to me that *Theory of Justice* (1971) points towards the former and "Justice As Fairness: Political Not Metaphysical" (1985) points towards the latter.

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EXPLAINING GOVERNMENT GROWTH IN THE U.S. STATES

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Despite the explosive increase in the research program on government growth in recent years, little work has been done on government growth disaggregated to the subnational level. I examine the empirical validity of five competing models of government growth for the fifty U.S. states from 1945 to 1984: Wagner's Law, fiscal illusion, party control, bureau voting, and intergovernmental grant. Government size is defined in terms of state government spending as a proportion of total state economic output, with separate implicit price deflators being employed for the public and private sectors. Based on a longitudinal test of these competing models, the analysis uncovers strong empirical support for the bureau voting and intergovernmental grant models, moderately weak support for the Wagner's Law model, and virtually no support for the fiscal illusion and party control explanations. These findings have important implications for the study of government growth in general and, more specifically, in the states.

One of the outstanding features of U.S. political life in the postwar era has been the growth in the size of the public sector. While there has been general agreement about the existence of government growth, there is substantially less agreement surrounding extant explanations of the observed patterns of government growth since 1945. In recent years a myriad of alternative explanations of government growth have been proposed (Borcherding 1977; Larkey, Stolp, and Winer 1981; Lowery and Berry 1983; Tarschys 1975). Furthermore, numerous scholars have developed an extensive set of models of government growth and tested them competitively and empirically, both in the United States (Berry and Lowery 1984a; Kau and Rubin 1981; Lewis-Beck and Rice 1985; Lowery and Berry 1983) and in several nations (Cameron 1978; Mann 1980; Peacock and Wiseman 1967).

Despite the great volume of research written on government growth at the national level, little is known about patterns of government growth disaggregated to the subnational level. The small body of research that does exist suffers from numerous shortcomings: (1) general neglect of basic, rudimentary patterns of government growth over time; (2) a reliance on cross-sectional designs inappropriate for the study of dynamic government growth processes; and (3) a failure to develop alternative models of government growth and test them competitively. Before a more comprehensive understanding of the broad phenomenon of government growth can be achieved, we need to extend the research program to include subnational political systems as well.

I test competing explanations of government growth in the fifty U.S. states from 1945 to 1984. There are three major reasons why such a research endeavor is

AMERICAN POLITICAL SCIENCE REVIEW
VOL. 82 NO. 3 SEPTEMBER 1988

important. First, the states provide an excellent comparative setting within which the study of government growth can be conducted. The existence of fifty political systems with differing political, social, and economic contexts provides an excellent laboratory for the development and competitive testing of various models of government growth under different settings. Second, the study of government growth in the states is an important undertaking with value for students of state policy and politics. To date, the states have been virtually ignored as a focus of research on government growth. Currently very little is known about even basic patterns of government growth in the states, to say nothing about the empirical validity of state-level government growth models. Third, government growth in the states may have important implications for the overall growth of the public sector in the United States during the 1970s and 1980s. Such implications are particularly likely, given the New Federalism agendas of the Nixon and Reagan administrations and the associated transfer of some federal government responsibilities to the state and local governments. In other words, overall growth in the public sector may be driven more by state government growth patterns in recent years than in the past.

Competing Models of Government Growth

Several general models of government growth found in the literature can be applied to the growth of the public sector in the states. Before I describe these models, three caveats should be noted. First, the set of government growth models presented below does not constitute an all-inclusive list. Several models cited in the literature cannot be readily applied to the states, either for theoretical reasons or because the lack of available data prevents an empirical test of the explanation. Second, the model specifications presented below are very simple. I would argue, however, that such simple specifications are in keeping with the theoretical orientations presented throughout the literature and that a competitive test of these simple models allows the researcher to develop a baseline model or set of models upon which more comprehensive multivariate models can be built. Third, theoretical processes suggested by these competing models need not be mutually exclusive. It is possible that some of the independent variables included in these separate models are interrelated theoretically and empirically and that the specification of the relationship between one independent variable and government growth carries with it an implied specification of some other relationship.

Wagner's Law

In an early classic work on government growth, Wagner (1877, pt. 1) posited that government growth was a function of factors associated with increased industrialization, economic affluence, and population growth. These factors were seen as being associated with government growth for two reasons: (1) demand for government goods and services was viewed as income-elastic (i.e., demand increased at a faster rate than did economic affluence); and (2) increased industrialization and population were assumed to be associated with increased levels of societal interdependencies and externalities requiring increased governmental regulation and intervention. A complete specification of Wagner's Law similar to that tested by Lowery and Berry (1983) would require the inclusion of variables for industrialization, income, population, and population density in a model of government size. Based on the limited availability of such data across the fifty states, the following specification of Wagner's Law can be tested:

$$\text{GOVSIZE}_t = a + b_1(\text{INCOME}_{t-1}) + b_2(\text{POPUL}_{t-1}),$$

where GOVSIZE is the ratio of state government expenditures to total output of each state's economy in the year t; INCOME is the value of total personal income in each state; and POPUL is the total state population. If Wagner's Law is supported empirically, one would expect both coefficients to be in the positive direction.

The Fiscal Illusion Explanation

A second explanation suggests that government growth can be attributed to a reliance by state governments on forms of revenue collection that hide the costs of providing public goods. Such a revenue system, with its emphasis on hidden taxes, causes citizens to underestimate the cost of governmental goods and services and hence demand more than they would if they could accurately estimate these costs (Goetz 1977).

Several aspects of a revenue system are seen as having illusory effects. First, withholding provisions, which involve the government's deducting taxes from citizens' paychecks, are seen as illusory (Enrick 1964). Second, some have argued that indirect taxes (such as corporate income taxes) have an illusionary (and hence expansionary) impact because (1) they are merely passed on to consumers as increased prices and (2) citizens are unable to estimate the "tax element" of the price they pay for private goods (Goetz 1977). Third, increased complexity of the tax system (as characterized by reliance on many different taxes) is seen as illusory, primarily because taxpayers are less likely to be able to comprehend its complexity and estimate the "true" costs of government services effectively.

It is difficult to specify in one simple but comprehensive model all of the illusory aspects of a state's tax system. However, in this analysis I rely on a derivative of the fiscal illusion model presented by Lowery and Berry to provide a partial test of the fiscal illusion explanation of government growth:

$$\text{GOVSIZE}_t = a + b_1(\text{INDTAX}_{t-1}) + b_2(\text{CORPTAX}_{t-1}) + b_3(\text{COMPLEX}_{t-1}),$$

where INDTAX is the proportion of state government revenues collected through the personal income tax, which relies on the withholding mechanism; CORPTAX is the proportion of state government revenues collected through corporate income taxes, representing the indirect tax illusion; and COMPLEX denotes the complexity of the state tax system. It is expected that all coefficients will be in the positive direction.

The Party Control Explanation

The party control explanation posits that government growth is related systematically to control of governmental policy-making institutions by the liberal (usually the Democratic) party within the state political system. Shifts in control of the governor's office and state legislature in the Democratic (i.e., liberal) direction are expected to result in increases in state expenditures as a proportion of the total state economy; while shifts in the Republican (i.e., conservative) direction are expected to result in either decreases or smaller increases in relative government size.

The thesis that party control of state government has important policy implications is not universally accepted. Such a thesis makes the crucial assumption that the party system within a state is policy-differentiated—that is, that the competing political parties-in-government take on different policy positions (Garand 1985). In other words, it is assumed that party labels have policy relevance and that the political parties within a state political system stake out significantly different positions on issues relevant to state expenditure decisions. The degree to which this assumption is validated empirically in the states is subject to considerable debate (Dawson and Robinson 1963; Downs 1957; Garand 1985; Winters 1976). However, it would seem that an empirical test of the party control explanation is in order; this explanation can be specified as follows:

$$\begin{aligned}\mathrm{GOVSIZE}_t = a &+ b_1(\mathrm{GOVSIZE}_{t-1}) \\ &+ b_2(\mathrm{GOVERNOR}_{t-1}) + b_3(\mathrm{LEGIS}_{t-1}) \\ &+ b_4\,(\mathrm{GOVERNOR}_{t-1} \cdot \mathrm{LEGIS}_{t-1}),\end{aligned}$$

where GOVERNOR is a binary variable indicating Republican control of the governor's office and LEGIS is the degree of Republican control of the state legislature. If the party control model is supported empirically, one would expect the coefficient for the lagged value of GOVSIZE to be positive and all three coefficients for the party control variables to be in the negative direction.

The Bureau Voting Explanation

This explanation ties government growth to the advantages in voting power that government employees have over other citizens. This thesis assumes that government employees are self-interested actors who desire increased government spending for their own well-being (Niskanen 1971). Further, it is assumed that government employees are more likely to turn out to vote (Bush and Denzau 1977) and that subsequently the spending preferences of government employees—reflected in the candidates advantaged by government employee vote choices—are more likely to have an impact on public policy than the spending preferences of other citizens. Although the direct empirical evidence is not very supportive of this explanation (Courant, Gramlish, and Rubinfield 1980; Kau and Rubin 1981; Lowery and Berry 1983), the model has not been tested in a full range of political systems. Hence, the model can be specified as follows:

$$\mathrm{GOVSIZE}_t = a + b_1(\mathrm{GOVEMP}_{t-1}),$$

where GOVEMP is the number of government employees as a proportion of state population.[1] If the bureau voting explanation is to be validated empirically, it is expected that b_1 will be greater than zero.

Intergovernmental Grant Explanation

This explanation attributes government growth in the states to the influx of intergovernmental grants from the federal government. Whether or not intergovernmental grants have contributed to growth in the size of state governments is subject to some debate. Some scholars argue that intergovernmental grants provide state governments with additional dollars and that the influx of these dollars contributes to growth in the size of the state public sector relative to the size of the total state economy. Others posit that intergovernmental grants perform a replacement function; that is, such grants are utilized to reduce the state's revenue burden while at the same time allowing the state to maintain its current spending levels. In order to estimate the impact of intergovernmental grants on state government growth, the following model can be specified:

$$\mathrm{GOVSIZE}_t = a + b_1(\mathrm{FEDGRANT}_{t-1}),$$

where FEDGRANT is federal grants-in-aid to state governments as a proportion of total state expenditures. If this explanation is correct, it is expected that b_1 will be positive.

Data and Methods

The data employed in this study were collected as part of a long-term project on dynamic policy processes in the states (Garand 1985, 1987; Lowery, Konda, and Garand 1984). With only a few exceptions, annual data for each of the variables in this analysis were collected for the 40-year time period from 1945 through 1984. A listing of the variables and a brief description of their empirical indicators used to estimate the parameters of the government growth models are presented in Table 1.

Table 1. Description of Empirical Indicators Used to Test Competing Models of Government Growth

Explanation	Variable	Description of Indicator
	$SEXPEND_t$	total state government expenditures
	$INCOME_t$	total state personal income
	$SLDEF_t$	implicit price deflator for state and local government purchases of goods and services (1972 = 100) (*source:* Bureau of Economic Analysis, U.S. Department of Commerce)
	$GNPDEF_t$	implicit price deflator for gross national product (1972 = 100) (*source:* Bureau of Economic Analysis, U.S. Department of Commerce)
	$GOVSIZE_t$	ratio of total state expenditures (deflated by the implicit price deflator for state and local government purchases of goods and services) to total state personal income (deflated by the implicit price deflator for gross national product) $GOVSIZE_t = \frac{SEXPEND_t/SLDEF_t}{INCOME_t/GNPDEF_t}$ $= \frac{SEXPEND_t}{INCOME_t} \; \frac{GNPDEF_t}{SLDEF_t}$
Wagner's Law	$INCOME_t$	total personal income (deflated by the implicit price deflator for gross national product)
	$POPUL_t$	total state population
Fiscal illusion	$INDTAX_t$	total state personal income taxes divided by total state revenue
	$COMPLEX_t$	Herfindahl index of revenue concentration (see Wagner 1976)
	$CORPTAX_t$	total state corporate income tax divided by total state revenue
Party control	$GOVSIZE_{t-1}$	lagged measure of government size
	$GOVERNOR_t$	1 = Republican control of the governor's office; 0 = Democratic control of the governor's office
	$LEGIS_t$	1 = Republican control of both chambers of state legislature; .5 = split control of the state upper and lower houses; and 0 = Democratic control of both chambers of state legislature
Bureau voting	$GOVEMP_t$	total number of state and local government employees divided by total state population
Intergovernmental grant	$FEDGRANT_t$	total federal intergovernmental grants-in-aid to state governments divided by total state expenditures

The Dependent Variable

The dependent variable (GOVSIZE) measures the level of state government spending as a proportion of total state economic output. Two major issues are raised by such a strategy for the measurement of government size. The first issue pertains to the indicator employed to measure the size of the total state economy. At the national level, most studies of government growth utilize a measure of the gross domestic product (GDP) or gross national product (GNP) (Berry and Lowery 1984a; Lewis-Beck and Rice 1985; Lowery and Berry 1983; Mann 1980). However, there is no equivalent measure of total economic output available systematically across all fifty states for the 40-year time period of this analysis. I utilize total state personal income as a surrogate measure of state economic output. While such a measure is not a perfect indicator, patterns of state personal income over time should be relatively sensitive to changes in overall state economic output over time, and one can argue that such an indicator reflects generally the underlying concept of the size of each state's economy.

The second major issue concerns the use of differential price deflators for the public and private sectors. Many scholars (Cameron 1978; Lewis-Beck and Rice 1985) assume that the price deflators for government spending and the private economy are identical. In measuring government growth, these authors do not deflate the dollar amounts used in the numerator and denominator, primarily because to do so would involve multiplying the ratio of government spending to total economic output by a value of one (i.e., CPI/CPI). However, other scholars have argued that this strategy for measuring government size fails to take into account the differential price deflators characterizing the public and private sectors (Beck 1976; Berry and Lowery 1984a, 1984b; Garand 1987; Lowery and Berry 1983). According to Lowery and Berry (1983, 680), the size of government (relative to the magnitude of total economic output) may grow for two reasons: "(1) the scope of government activity may broaden (i.e., government may increase the amounts and types of goods and services it provides), and (2) the cost of providing a *constant* level of goods and services may rise relative to the prices of goods and services in the private sector" (emphasis original). In other words, if the inflation rate is higher in the public sector (i.e., if prices rise at a faster rate in the public sector), the share of total economic output going to the governmental sector will increase even if the scope of government activity remains constant. Government can grow as a result of the higher relative cost associated with goods in the public sector when compared to the cost of providing goods in the private sector. Hence it is necessary to adjust the government size measure to take into account the differential price deflators characterizing the public and private sectors. This can be done by measuring government size following the convention utilized by Lowery and Berry (1983):

$$\mathrm{GOVSIZE}_t = \frac{\mathrm{SEXPEND}_t/\mathrm{SLDEF}_t}{\mathrm{INCOME}_t/\mathrm{GNPDEF}_t}$$

$$\mathrm{GOVSIZE}_t = \frac{\mathrm{SEXPEND}_t}{\mathrm{INCOME}_t} \quad \frac{\mathrm{GNPDEF}_t}{\mathrm{SLDEF}_t}$$

where SEXPEND is state expenditures; SLDEF is the implicit price deflator for state and local government purchases of goods and services (1972 = 100); INCOME is total state personal income; and GNPDEF is the implicit price deflator for gross national product (1972 = 100).[2]

The Independent Variables

The description of empirical indicators for the independent variables is presented in Table 1 and is generally straightforward. Several issues and caveats should be noted. First, in several cases data constraints either make impossible the measurement of indicators of all variables relevant for the government growth models or prevent the specification of a full range of models similar to that presented by Lowery and Berry (1983). Second, some of the data are not available for all years in the analysis; data on state government employment necessary for testing the bureau voting explanation are available only from 1952 to 1984. Third, in all cases the independent variables are lagged to capture the lag time between state policy decisions and their outcomes. It would be inappropriate to specify government growth models that suggest that current government size is a function of current conditions, given the fact that decisions underlying government size are made prior to the actual policy outcome.

Model Estimation

Each of the government growth models is estimated separately for each of the fifty states utilizing ordinary least squares (OLS) regression. Unfortunately, time series models like those presented here are often contaminated by the effects of autocorrelated error terms—a form of model misspecification that renders OLS regression inappropriate (Ostrom 1978). In order to correct this problem, a two-stage diagnostic and correction procedure is utilized (Hibbs 1974; Ostrom 1978).[3] The end result is a series of parameter values uncontaminated by the effects of autocorrelated error terms.

Empirical Results

Because of space limitations, it is not possible to present the complete parameter values for all of the models examined in this study. Instead, Table 2 presents a summary of these results for the five government growth models tested.[4] This table indicates the degree to which the coefficients associated with each of the models are (1) in the direction consistent with theoretical expectations and (2) of a magnitude sufficient to suggest an adequate level of substantive significance.[5]

Wagner's Law

The results presented in Table 2 suggest substantial variation across the states in the level of empirical support for Wagner's Law. First—and contrary to expectations—in the results upon which Table 2 are based a majority of the coefficients for INCOME (28 out of 50, or 56%) and several of the coefficients for POPUL (6 out of 50, or 12%) are in the negative direction. For the states with negative coefficients for the INCOME variable, the results suggest that demand for public goods is income-inelastic (i.e., increased income is not associated with greater demand for public goods relative to demand for private goods). The negative coefficients observed for the POPUL variable in some states indicate that increased societal interdependencies associated with higher population do not—as it was expected they would—coincide with higher levels of governmental spending relative to the size of the total state economy. Overall, there is a significant number of coefficients that deviate from the expected direction, though a majority (66 out of 100) are in a direction consistent with expectations. Second, when the stricter (though arbitrary) criterion—namely, that the coefficient be in the proper (positive) direction and exceed the value of its standard error—is utilized, one finds that 34% (17 out of 50) of the INCOME coefficients, 78% (39 out of 50) of the POPUL coefficients, and 56% (out of 100) of all coefficients meet this standard. Third, in many cases states with positive coefficients for one of the independent variables exhibit negative coefficients for the other.

Table 2. Applicability of Government Growth Models to Data from Individual States

State	Bureau Voting	Intergovernmental Grant	Wagner's Law	Fiscal Illusion	Party Control
Alabama	*	*		*	
Alaska	*				
Arizona		*			
Arkansas		*			
California	*	*			
Colorado		*			
Connecticut	*				
Delaware	*	*			
Florida					
Georgia	*	*			
Hawaii					
Idaho	*				
Illinois	*		*	*	
Indiana	*	*			
Iowa	*	*	*		
Kansas		*			
Kentucky	*	*			
Louisiana					
Maine	*	*			
Maryland	*	*			*
Massachusetts	*	*	*		
Michigan	*	*			
Minnesota	*	*			
Mississippi		*			
Missouri	*				
Montana	*	*			
Nebraska	*		*		
Nevada	*				
New Hampshire					
New Jersey	*				
New Mexico	*	*			
New York	*	*	*		
North Carolina	*	*			
North Dakota		*			
Ohio	*				
Oklahoma					
Oregon	*	*			
Pennsylvania	*	*	*		
Rhode Island	*	*	*		
South Carolina	*				
South Dakota	*	*	*		
Tennessee	*	*			
Texas	*	*			
Utah	*	*			
Vermont		*			*
Virginia	*	*			
Washington	*	*			
West Virginia	*	*			
Wisconsin	*	*	*		
Wyoming	*	*			
Proportion of slope coefficients in proper direction and exceeding one standard error	38-50 (.76)	35-50 (.70)	9-50 (.18)	2-50 (.04)	2-50 (.04)

Note: * means that all coefficients for model are in proper direction and exceed associated standard error.

Using an even stricter criterion, one finds that only 18% (9 out of 50) states have a set of two coefficients that meet this standard. Overall, then, the empirical support for Wagner's Law as applied to government growth in the states is, at best, quite modest. It is clear that the level of support for this explanation is not of a similar magnitude to that observed in Lowery and Berry's (1983) research at the U.S. national level.

Fiscal Illusion Explanation

In general, Table 2 suggests only weak inconsistent support for the fiscal illusion thesis. First, many of the coefficients are in the incorrect (negative) direction for INDTAX (16 out of 44, or 36%), CORPTAX (18 out of 46, or 39%), and COMPLEX (19 out of 50, or 38%); overall, 53 out of 140 coefficients (38%) are in the negative direction. Second, when one employs a stricter standard, one can observe that less than a majority of the coefficients for INDTAX (21 out of 44, or 48%), CORPTAX (12 out of 46, or 26%), and COMPLEX (20 out of 50, or 40%) are in the expected positive direction and exceed the value of their associated standard errors; overall, only 53 out of 140 coefficients (38%) meet these criteria. Furthermore, only in Alabama and Illinois are these standards met for all three independent variables; in all other states at least one coefficient is either in the negative direction or is relatively unstable (as indicated by the relative size of its standard error), though in 16 out of 50 states (32%) the standard is met for two out of three coefficients. Overall, then, the results presented here do not appear to offer much systematic support for the fiscal illusion thesis as an explanation of government growth in the states.

Party Control Explanation

An examination of summary findings presented in Table 2 indicates little support for the party control thesis of state government growth, once the impact of previous levels of government size are taken into account. First, a substantial number of coefficients are in the positive direction, indicating a higher level of government growth during periods of Republican party control of state governmental institutions. The numbers and proportions of negative coefficients are as follows: GOVERNOR (18 out of 45, or 40%), LEGIS (19 out of 30, or 63%), and the GOVERNOR · LEGIS (8 out of 23, or 35%). 23, or 61%). When the stricter standard is applied, one finds that very few coefficients are both negative and have magnitudes in excess of the value of their standard errors: GOVERNOR (4 out of 45, or 9%), LEGIS (6 out of 30, or 20%), and GOVERNOR · LEGIS (8 out of 23, or 35%). Finally, none of the states has a complete set of three coefficients that meet both criteria of negative valence and relative stability, although two states (Maryland and Vermont) have their only coefficient meeting these standards. Clearly, very little empirical support is uncovered for the party control explanation of government growth in the states. It would appear that there is no universal tendency for states to exhibit higher levels of government growth during eras of Democratic control of state policy-making institutions.

Bureau Voting Explanation

The results depicted in Table 2 are generally strongly supportive of the bureau voting model of state government growth. First, 45 out of 50 coefficients (90%) are in the expected positive direction. Second, 38 out of 50 coefficients (76%) are positive and exceed the value of their associated standard errors. In other words, in three-fourths of the states the relationship between government employment and government size is positive and stable enough to be considered as providing adequate support for the bureau voting model. All in all, there appears to be substantial support for the bureau voting explanation of state government growth.

Intergovernmental Grant Explanation

The intergovernmental grant explanation suggests that government growth is a function of the influx of intergovernmental grants from the federal government, which are (the thesis posits) utilized by state governments to increase (in a cost-free manner) the level of public goods provided in that state. The summary of empirical results shown in Table 2 suggests support for this model. First, 46 out of 50 coefficients (92%) are in the expected positive direction, indicating that increases in federal grants as a proportion of state self-generated revenue is associated with higher levels of overall government size. Furthermore, when one includes a criterion of relative coefficient stability, one finds that 35 out of 50 parameter values (70%) are both positive and are larger in magnitude than their associated standard errors. It would appear that for over two-thirds of the states the infusion of intergovernmental grants has contributed a relatively stable positive impact to the growth of state government's share of total state economic output.

Overview

Based on Table 2 and the foregoing discussion, it is clear that there is substantial variation in the degree of support for competing models of government in the states. The bureau voting and intergovernmental grant models appear to provide the closest and most consistent approximations of observed patterns of state government growth over time, while the Wagner's Law explanation seems to generate modest levels of empirical support. The fiscal illusion and party control explanations find little support whatsoever. It should be noted that for a significant number of states more than one model provides an adequate level of explanatory power to the patterns of government growth over time. In some states as many as four models are supported by the empirical analysis. There are two potential reasons for this. First, it is possible that some of the competing explanations are not mutually exclusive, and that the independent variables represented in the competing models are highly correlated and empirically indistinct. Alternatively, it is possible that several sets of explanations serve as distinct components that act in an additive (or perhaps interactive) manner in influencing government size and must therefore be built into a comprehensive multivariate model for each state. Future research will explore these alternatives in developing a more complete model of government growth in the states.

Conclusion

I have explored the determinants of government growth in the fifty states. The models estimated give a relatively clear view of the relative merits of competing models of state government growth. Some of the models of government growth must either be revised or eliminated from the research program as it applies to the states. The party control and fiscal illusion models, while plausible on theoretical grounds, do not appear to provide systematic explanations of state government growth. On the other hand, the bureau voting and intergovernmental grant models seem to be well supported empirically, while Wagner's Law is supported only modestly. Interestingly, these findings stand in sharp contrast to those of Lowery and Berry (1983), who find that Wagner's Law provides the best explanation of federal government growth from 1948 to 1979. In the present analysis, the truncated specification of Wagner's Law finishes a poor third in terms of its empirical validity in the states.

The strong empirical support found for the bureau voting explanation suggests some important implications for the working of Democratic government in the states. Interestingly, this model is cited by Lowery and Berry (1983, 672–75) as being an "excessive government" explanation of government growth, indicating that the process reflected in this model assumes that "the institutions of government are far from neutral in determining the size of the public sector. In particular, this set of explanations assumes that these institutions operate to expand the government's share of the economy beyond the size demanded by the public." Furthermore, one can plausibly suggest that the intergovernmental grant model also falls into the category of excessive government explanation, given that the influx of federal grants may result in increases in the size of government beyond that demanded by the public. In any event, if these models are supported empirically (as they appear to be), they may lend support to the view that state governments are composed of self-interested actors who utilize at least some of the tools at their disposal to increase the relative size of the state public sector.

Notes

This research is funded in part by a Summer Faculty Research Grant provided by the Council on Research, Louisiana State University, Baton Rouge. I am grateful to Michele Rome, Amanda Jones, and Brent Bourgeois for assistance in collecting the data utilized in this analysis; to Julie DeJean for her superb typing; and to Dave Lowery, Ron Weber, and Harvey Tucker for constructive comments.

1. The bureau voting thesis suggests that the relative strength of government employees in the electorate should have a positive impact on state government growth. However, data on the size of the electorate for each state-year case are not readily available, so I employ population in the denominator as a proxy for the size of the electorate. While the measure of bureau voting strength employed in this model is not perfect, it does (I believe) capture the general concept.

2. The implicit price deflator for state and local government purchases of goods and services is not measured separately for each state-year case. Instead, this is a general price deflator appropriate for state and local spending as a whole. The values of this index are the same across all states for any given year. While a separate price deflator for each state would be ideal, I am aware of no such indicator available for the full time period under study.

3. First, the autocorrelation function (ACF) and the partial autocorrelation function (PACF) for each model are examined in order to discern the existence and order of the underlying autoregressive process. The second stage involves the proper estimation of parameter values for the government growth models. If a pattern of autocorrelated error terms is not observed, OLS parameter estimates are used, for they accurately estimate the parameters of the regression model. If, on the other hand, autocorrelated error terms are in evidence, a two-step full-transform method equivalent to generalized least squares (GLS) is employed. For the party control model the inclusion of a lagged endogenous variable (GOVSIZE_{t-1}) makes the two-step full-transform method inappropriate. In order to provide parameter values uncontaminated by serial correlation, an instrumental variable–pseudo-GLS estimation procedure is employed. This is the same procedure utilized in the government growth research by Lowery and Berry (1983) and is described in detail by Ostrom (1978). However, in only two of the fifty states (New York and Tennessee) does the party control model exhibit patterns of autocorrelated error terms, so it is only in these two states that the special estimation procedure is employed.

4. The tables reporting the complete set of parameter values upon which Table 2 is based are available upon request from the author.

5. In the tables upon which Table 2 is based, I report unstandardized regression coefficients, their standard errors, and their probability values. However, the use of significance tests to make inferences about population parameters is the subject of a substantial amount of debate in the social sciences. One might argue that the data used in this analysis constitute the population of observations from 1945 to 1984 for each state. Because unstandardized regression coefficients reflect actual population parameters (and not *estimates* of those parameters), one might argue that this makes unnecessary the use of standard significance tests. On the other hand, it is recognized that many social scientists find that significance tests are appropriate, or at least helpful, in evaluating relationships within populations. In deference to this position and primarily for illustra-

tive purposes I present significance test results for parameter values in the tables used to construct Table 2 (available upon request).

If one does not employ tests of statistical significance in evaluating models involving population data, what criteria can be utilized to evaluate the parameter values of these models? What is necessary is a criterion that gives substantive significance to the parameter values associated with the variables for these government growth models. In evaluating these parameter values, I employ an (admittedly) arbitrary evaluative criterion: if a coefficient exceeds the value of its standard error, I assume that the coefficient represents a stable impact of the independent variable on government size and hence that it can be characterized as indicating an adequate level of substantive significance. The standard error of an unstandardized regression coefficient reflects the stability (or consistency) of the impact of a one-unit shift in the independent variable, controlling for the effects of any additional independent variables. This is important because one's confidence in the substantive significance of a coefficient increases as the level of fluctuation in the impact of the independent variable declines. While this standard is less strict than that employed by scholars using conventional significance tests to evaluate the empirical validity of models, it does represent one plausible indicator of the degree to which one can be confident in the strength of the findings.

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SYMPOSIUM

THE RETURN TO THE STATE

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Three important questions are raised by the "return to the state" movement of recent years. First, are the pluralist, structural functionalist, and Marxist literatures of political science societally reductionist, as this movement contends? Second, does the neostatist paradigm remedy these defects and provide a superior analytical model? Third, regardless of the substantive merits of these arguments, are there heuristic benefits flowing from this critique of the literature? Examination of the evidence leads to a rejection of the first two criticisms. The answer to the third question is more complex. There is merit to the argument that administrative and institutional history has been neglected in the political science of the last decades. This is hardly a "paradigmatic shift"; and it has been purchased at the exorbitant price of encouraging a generation of graduate students to reject their professional history and to engage in vague conceptualization.

Arguments for a return to the concept of the state have been made intermittently ever since Frederick Watkins (1968) prematurely solemnized its burial 20 years ago. The writers who have advocated a return to the state have had different goals and agendas. In the cases of Nettl (1968), Stepan (1978), and Nordlinger (1981) the argument seems to be one of emphasis. The campaign carried on under the auspices of the Committee on States and Social Structures of the Social Science Research Council to "bring the state back in" speaks of "paradigmatic shifts" (Evans, Rueschemeyer, and Skocpol 1985). The "pluralist-functionalist" and Marxist paradigms, which are described as the waning paradigms of contemporary political science, are said to be societally reductionist, according no autonomy to state structures and politics and hence fundamentally lacking in explanatory power. The statist paradigm remedies this defect and will replace these earlier approaches.

I respond to three questions raised by this polemic. First, are the pluralist, structural functional, Marxist, and other literatures of political science societally reductionist? Second, does the "statist paradigm" remedy these defects? Third, regardless of the substantive merits of these arguments, are there heuristic benefits flowing from this critique of the literature?

Arguments for a Return to the State

I propose to review these criticisms, and make a search of the literature to determine the extent to which this view of "mainstream" political science is sustained by evidence. I cover ground similar to that covered by G. David Garson (1974) in an article published more than 10 years ago and in a subsequent book (1978). He provided a review of the history of interest group theory using as his data base all of the articles and reviews which had appeared in this *Review* since it began publishing in 1906. He also included some 20 frequently cited books dealing with group theory. His principal conclusion was that political science "grows" largely reactively, in response to outside stimuli: "the rise of economic groups, the tides of progressivism and disillusion, the birth and excitement of modern psychology," and the like (p. 1519), rather than through the self-conscious testing of analytical models.

Using group theory as exemplifying this disciplinary propensity, he pointed to the early polemic between the "classic" sovereignty and "classic" pluralist theorists. The classic theory of sovereignty overstated its case, although if one looks carefully at this nineteenth-century literature one will find careful qualifiers of the plenary power of the state. The assertion of the absolute authority of the state ran into the anomalies of the empirical and normative limits on state authority—both the conservative ones of religion and property and the liberal ones of the rights of association, assembly, petition, and the like. Classic pluralism similarly overstated its case, rejecting the very notion of state sovereignty, and characterizing the state as one association among many. By denying any sovereignty whatever to the state, Laski (1917) and the other early pluralists deprived themselves of an agency capable of remedying injustice and providing basic security. Garson points out that the history of political science would have been quite different if these alternatives had been formulated as analytical models, as ideal types, rather than assuming that they modeled reality or that reality ought to be made to conform to them. The contemporary statists in their posing of the pluralist and statist alternatives make a similar mistake.

In a more recent article Andrew McFarland (1987) reviews the history of interest group theory in the period since World War II. Prior to the mid-1960s, he points out, "the work of Dahl, Truman, Lindblom, and others provided a coherent and convincing theory of power in America. Dahl's polyarchy and the implicit 'economic theory of democracy' provided a theory of elections. Truman's description of American institutions and politics as a complex texture of decentralized bargaining among a myriad of interest groups was readily assimilated into Dahl's pluralist discussion of power. Lindblom's theory of incremental decision making showed why policies emerging from decentralized bargaining within a polyarchy might be both more effective and representative than policies emerging from central government direction" (p. 129).

The disillusionment and demoralization in U.S. politics in the 1960s and 1970s shook the credibility of this model; and in its place a "plural-elitist" model, developed in the work of Lowi (1969) and others, came to be an influential view in the discipline. In this model special interests captured particular areas of public policy, forming "iron triangles" or "subgovernments," not subject to popular control. This pluralist revisionism of the 1970s and 1980s reacted only to the interest group theory of the 1950s and 1960s, and as I shall suggest it failed to register the fact that its pessimism was similar to the pluralist pessimism of the 1930s, which was based on a many-times replicated appreciation of the fact that the power of business interests was substantially more equal than other interests.

A third model associated with the work of James Q. Wilson (1980), McFarland calls the "triadic model." In this model the policy process is viewed as specialized by issue areas. Normally interest groups mobilizing to lobby the government in a particular direction are opposed by countervailing groups, and "state agencies are normally assumed to have a significant degree of autonomy . . . as well as a capacity to give some coherence and continuity to the system" (p. 141). The triad consists of the two interest group camps and an autonomous set of state agencies, varying across issue areas.

The important point made by Garson and McFarland from the perspective of the present statism-pluralism polemic is that with the exception of the classic pluralism interlude of the turn-of-the-century period and a few formulations in the 1950s and 1960s (Latham 1952; Golembiewski 1960), the various versions of interest group theory have operated with some measure and form of governmental autonomy.

The intellectual history that I shall review is readily accessible. In an article, "The State," published in the original *Encyclopedia of the Social Sciences,* the political theorist, George Sabine complained a bit about the concept. "The word commonly denotes no class of objects that can be identified exactly, and for the same reason it signifies no list of attributes which bears the sanction of common usage. The word must be defined more or less arbitrarily to meet the exigencies of the system of jurisprudence or political philosophy in which it occurs" (1934, 328). Thirty-four years later in the successor compendium of social science knowledge, *The International Encyclopedia of the Social Sciences,* Frederick Watkins observed that the complexity of the interaction of government and people is such that "political scientists prefer to use other terms in describing the phenomena that once was subsumed under the concept 'state' " (1968, 156).

Watkins acknowledges that the concepts of the state and sovereignty made sense in the century-and-a-half between the Treaty of Westphalia and the French Revolution, during the Age of Absolutism, when Louis XIV could say "l'Etat c'est Moi," and Frederick II could speak of himself as "der erste Diener des Staates." Democratization led to a devaluation of the normative concept of the state. In a democratic society it is hard to distinguish the state from the citizens in whose name its authority is being exercised: "Sovereignty thus conceived is little more than a bloodless legal fiction. Attention shifts from the state to the government, which though it makes no claim to sovereignty, does all the actual ruling" (p. 153).

If there is any point in the use of the concept, according to Watkins, it is in the Weberian sense "that the distinctive feature of the state, as compared with other associations, is its attempt to monopolize coercive power within its own territory. . . . From the standpoint of a purely descriptive political science it is sufficient, therefore, to define the state in terms of the limit, and to study the conditions that accompany the greater or lesser degrees of monopoly that have been achieved in particular times and places. The difficulty is that it places excessive emphasis on the coercive aspects of political life" (pp. 153–54).

As the state concept fell into disuse in mainstream political science it was replaced by such terms as *government,* and later by *political system.* The tendency to abandon the state concept and replace it by other concepts was attributable to the enormous political mobilization that took place in the Western world in the nineteenth and twentieth centuries and the proliferation of new political institutions —political parties, pressure groups, the mass media, and the like—that accompanied it. The concept of the political system included the phenomena of the state —the legally empowered and legitimately coercive institutions—but it also included these new extralegal and paralegal institutions of political parties, interest groups, media of communication, as well as social institutions such as family, school, church, and the like, insofar as they affected political processes. Political system theory and structural-functionalism were not reductionist of the state and governmental institutions. They grew out of a realism that recognized the processual character of politics, and examined institutions—legal, paralegal, and informal—in terms of what they actually did. Structural-functionalism was just that; and system theory was a formal acknowledgement that the political process was a set of interdependent subprocesses.

For the Marxists and neo-Marxists, however, the state continued to be a central concept, the instrumentality through which the capitalist class dominated the social order. For the Marxists the assimilation of the state into the political system and its disaggregation into a host of interacting phenomena in mainstream political science represented a fudging of the reality of class struggle. As I shall suggest, for the Marxist intellectual tradition the relation of the state to class struggle continues to the present day to be a central polemical issue. And Skocpol's notion of the autonomy of the state has to be seen in the context of that Marxist, neo-Marxist, and post-Marxist polemic.

The first major assault on this abandonment of the state concept in mainstream political science was that of J. P. Nettl, whose article, "The State As a Conceptual Variable" (1968) appeared in the same year as the publication of Watkins's encyclopedia article. Nettl explained the neglect of the concept of the state in Britain and the United States, and particularly in the United States, by the fact that the phenomena of "stateness" are weak in those countries. But the general point made by Nettl regarding the differing salience of the state in the United States and in continental Europe has been appreciated at least since Tocqueville and has been part of the conventional wisdom of comparative government and politics. Though he advocates a return to the state concept, he does not effectively deal with Sabine's and Watkins's uneasiness about the ambiguity of the term.

He offers a definition of the state with four components. First, "it is a collectivity that summates a set of functions and structures in order to generalize their applicability" (1968, 562). It is not clear which structures and which functions Nettl includes in this collectivity, although he refers to bureaucracies, parliaments, and even political parties. Second, the state is a unit in international relations; all independent nations, whether having strong or weak states, have this unified international aspect. Third, the state is autonomous; it is a distinct sector of society. And fourth, the state is a sociocultural phenomena, by which Nettl means that the individual members of the state have a generalized cognition and perception of it.

"Stateness," according to Nettl, is a quantitative variable, and the strength and weakness of the state in individual societies can be compared through functional analysis. Nettl includes among these functions, that of central administration, which varies in strength and scope across countries. Thus Anglo-American central administration is weaker than the continental European pattern. Law enforcement patterns differ in strong and weak states; thus in Britain and the United States law and law enforcement have more autonomy than is the case in continental Europe. The legal professions are more clearly independent of government. He also suggests the hypothesis that the strength and the weakness of central administration are related to the strength and weakness of party systems.

Nettl's contribution is an argument to go beyond the mainstream Anglo-American view of the state as the "general area of central government in contradistinction to society" (p. 591) and to examine the proposition that "more or less stateness is a useful variable for comparing western societies, and that the absence or presence of a well-developed concept of state relates to and identifies important empirical differences in these societies" (p. 592).

When all is said and done, Nettl has disaggregated the state into a variety of components, which he treats as quantifiable and measurable variables. The all-important question for his thesis, how the state concept relates, aggregates, summates, integrates—and so on—such phenomena as bureaucracies, courts, armies, parliaments, parties, interest groups, media of communication, and public opinion is not answered by Nettl. What political phenomena does it include? What does it exclude? How should one weight these components? Like Sabine and Watkins, Nettl is telling us that to measure stateness it is necessary to operationalize and disaggregate the concept. What the state is depends on which operational measures one decides to use. Nettl's argument that nations vary in their degree of stateness, by which he means the scope and extent of governmental power and authority, was similar to the arguments then being made by political scientists using other terms such as *public policy, governmental output,* and *governmental performance.* Lowi's threefold classification of policy areas—regulative, distributive, and redistributive—had already been on the table for four years at the time of Nettl's writing and was soon to be used by the former in cross-national comparison. The comparative historical, and econometric public policy movements were getting under way in the middle and late 1960s; and they would successfully bring to earth those cross-national contrasts that Nettl was emphasizing in his notion of "stateness."

Another predecessor claimed by the contemporary statist movement is Alfred Stepan whose *State and Society: Peru in Comparative Perspective* appeared in 1978. Stepan is also critical of the "reductionism" of the liberal pluralist and classic Marxist approaches to the state and politics. But he does not go quite as far as the more recent statists. While his area specialty is Latin America, his reductionist critique is addressed to comparative politics generally. The particular point he makes with respect to Latin American studies is that there is an alternative body of theory—the organic-statist-corporatist theoretical tradition—that is more appropriate for the analysis of Latin American and Ibero-European politics, since this theoretical tradition developed in response to the institutional traditions and characteristics of these areas. He describes the liberal-pluralist approach to the state as integrally individualist and as fully committed to the doctrine that there can be no "general good" other than that which flows from the pursuit of the interests of individuals: "A methodological and normative assumption among both political and economic thinkers in the liberal pluralist tradition is that it is undesirable to use the concept of the general good" (p. 7). Stepan also argues that liberal-pluralism treats the state as a dependent variable, that such theories tend systematically to draw attention away from viewing the state as an autonomous agency. This distortion of liberal pluralism in Stepan's work is shared with the contemporary statist movement, and I will return to this question of the place of the general welfare, the state, and the government in pluralist theory and research. It should, however, be pointed out that Stepan's book reflects a more substantial search of the literature than the later work in the new statist tradition.

It is not clear where to locate Eric Nordlinger in this polemic (1981). He stops short of unfurling a new paradigmatic banner (1987). He is empirical and "positivist" to the core. In asserting the autonomy of the state he by no means excludes the power of societal factors. Rather he attributes a societal reductionism to his mainstream colleagues and represents his own work as an important effort to carve out a place for the initiative of governmental officials in the making of public policy. If one corrects for his misreading of the pluralist tradition, there is no change of paradigm here but rather a research program of considerable promise intended to distinguish among polities according to the degree to which state (governmental) personnel take the initiative in the making of public policy and the factors and conditions that explain these differences in degree. I do not believe that a research program of this kind would strike the typical political science practitioner as a revolutionary departure from mainstream research designs, which, as I shall show, typically accord quite substantial policy initiative and autonomy to political executives, high-level bureaucrats, legislative committees and leaders, and the like.

Nordlinger avoids vague and amorphous formulations. Unlike the statists he offers a definition of the state in operational terms: "The definition of the state must refer to individuals rather than to some other kinds of phenomena, such as 'institutional arrangements' or the legal normative order. Since we are primarily concerned with the making of public policy, a conception of the state that does not have individuals at its core could lead directly into the anthropomorphic and reification fallacies. . . . Only individuals have preferences and engage in actions that make for their realization. And only by making individuals central to the definition can Hegelian implications (substantive and metaphysical) be avoided when referring to the state's preferences" (1981, 9).

Nordlinger has three other components of a definition of the state. The state should be defined to include not only the government and the bureaucratic agencies that derive their authority from it, "it should include all public officials—elective and appointive—at high and low levels—who are involved in the making of public policy" (1981, 10). It should avoid all characterizations that may vary from case to case, such as legitimacy and sovereignty. Finally, a definition should seek neutrality; it should avoid attributing specific functions and purposes to all states, such as the preservation of stability or the reproduction of capitalism.

With this carefully empirical set of defining characteristics, Nordlinger attacks what he alleges to be the "near unanimity . . . of all variants of liberal and Marxist writing on democratic politics" on the question of the dominance of societal constraints on the making of public policy. He refutes these imputed theoretical positions by asserting that there are three levels of state autonomy in policymaking and then illustrates them through an analysis of the secondary literature. The lowest level of state autonomy (type 3) refers to situations in which state and society preferences do not diverge and the state acts on its own preferences; that is to say, the particular policy adopted is the one preferred by state officials even though it does not encounter opposition from society. The second level of autonomy is one in which society's preferences differ from the state's preferences, but state officials persuade society to adopt state preferences. The third and highest level (type 1) is represented in situations in which the preferences of state and society diverge; but the state nevertheless enacts a policy consistent with its preferences. This extreme case, crucial to the support of Nordlinger's theory of state autonomy, is illustrated by analysis of the various ways in which the state can play on social divisions and employ the formidable powers of government. In a later paper (1987) Nordlinger offers a fourfold classification of state-society relations in terms of high and low state autonomy and societal support that has interesting research possibilities. Though Nordlinger fails to—and indeed cannot—document his characterization of liberal scholarship as societally reductionist, there is no doubt that he has made an important contribution to empirical theory. His analysis is rigorous and heuristic, full of interesting research leads.

Pluralism As Theory and in Empirical Research

The view associated with the Committee on States and Social Structures of the Social Science Research Council (SSRC) is a more integral critique of the so-called waning orthodoxies or paradigms of contemporary political science. These orthodoxies are "pluralism–structural functionalism" and Marxism. They are said to treat the state as a dependent variable; its actions are explained by the interplay of interest groups or of social classes. Thus Theda Skocpol, in her introduction to the first book published under the auspices of this committee, states that the previously dominant pluralist-functionalist approaches viewed government "primarily as an arena within which economic interest groups or normative social movements contended or allied with one another to shape the making of public policy decisions. Those decisions were understood to be allocations of benefits among demanding groups. Research centered on the societal "inputs" to government and on the distributive effects of governmental outputs. Government itself was not taken very seriously as an independent actor, and in comparative research variations in governmental organization were deemed less significant than the general functions shared by the political systems of all societies" (Evans, Rueschemeyer, and Skocpol 1985, 4; cf. Skocpol 1982).

This attribution of society-centeredness to the earlier literature is repeated at other points in this first book of the SSRC Committee (Evans, Rueschemeyer, and Skocpol 1985, chaps. 4, 10). Stephen Krasner, another committee member, comments along these same lines in a review article: "Pure interest group versions of pluralism virtually ignore public actors and institutions. The government is seen as a cash register that totals up and then averages the preferences and political power of societal actors. Government may thus be seen as an arena within which societal actors struggle to insure the success of their own particular preferences. The major function of public officials is to make sure that the game is played fairly. If public institutions are viewed as figurative cash registers or as literal referees, there is no room for anything that could be designated as a state as actor with autonomous preferences capable of manipulating and even restructuring its own society" (1984, 226). According to Krasner, where pluralists do recognize the initiating role of political leaders, they reduce state institutions to "individuals acting in roles" not restrained by institutional "imperatives and restraints." This is a more complex version of the reductionist argument; for Krasner it is an individualist as well as a societal reductionism.

In making the case for this reductionist treatment of the state and government in pluralism and structural functionalism neither Skocpol nor Krasner provides us with a literature search commensurate with the scope and variety of the political science literature about which they are generalizing. They seem to claim that the literature of political science may be subsumed under these two categories of pluralism–structural functionalism and Marxism. Would they include the substantial literature on Communist countries? Surely this literature, even that part of it that experimented with pluralist models, took governmental actors seriously, and dealt with the coercive, extractive, and regulative powers of government in addition to the allocative. Would an examination of the comparative government and politics literature and the country studies of Britain, France, Germany, Italy, and so on, with their emphasis on the power of political executives and bureaucracies, sustain Skocpol's complaint regarding the neglect of state agencies in mainstream political science? In U.S. studies how would they deal with the substantial literature on the presidency, the Congress, and the courts? How would they dispose of the flurry of publications on military regimes in third-world countries that appeared in the 1960s and 1970s?

If we look at the larger specifically pluralist literature, can we sustain this statist critique? Suppose we examine the ideas of the original statists in order to put both pluralism and statism into historical perspective. And then let us look at the empirical research done by the pluralists. Does it indeed conform to the society-centered reductionist pattern presented by Skocpol, Krasner, and their collaborators? Does it conform to the on-the-whole similar portraits presented by Nettl, Stepan, and Nordlinger?

It is sobering that although their central theme is the affirmation of statism as an alternative to pluralism, neither Skocpol nor Krasner nor the other antipluralists cited here seem to have looked into the original confrontation of statism and pluralism at the end of the nineteenth century and in the early decades of the twentieth. And although their argument for a return to the state rests on the claim that the pluralist empirical literature reduced the state and government to an arena and treated the government as a dependent variable (to the extent that it treated it at all) they simply fail to cite or to discuss the very substantial pluralist "interest group" literature of the last three quarters of a century.

It would not have taken a major search to raise serious questions about the validity of these statist characterizations. There are excellent reviews of the early statist and pluralist literature in the two encyclopedias of the social sciences—articles on the state by George Sabine (1934) and Frederick Watkins (1968) and articles on pluralism by Francis Coker (1934) and Henry Kariel (1968), all political theorists of repute. Sabine traces the emergence of the state concept from Machiavelli's *il stato* in *The Prince* and the first adoption of the term *secretary of state* for ministerial offices in the reign of Elizabeth. Its usage spread with the rise and diffusion of absolutism. And the notion of sovereignty developed along with it. The emergence of the state in the course of the sixteenth to nineteenth centuries was associated with expansion, aggression, and war making. As a process, the western European state arose out of a logic of aggression or defense against aggression. It was a consequence of the growth of armies and the recruitment and training of civil bureaucracies concerned with the extraction of resources, the procurement of supplies, and the regulation of behavior—all primarily in relation to this military-defense-expansion set of goals. The potential progressiveness of the state resulted from this concentration of power and its separation from the person of the ruler or rulers. As compared with the complicated dynastic, sacred, and secular particularisms that preceded it, it had the potentiality for providing greater personal security, a larger internal market, and perhaps improved systems of law and justice.

The notion of state sovereignty survived the crises of the democratic revolutions. What happened was that the "people" became sovereign or "parliament" became sovereign. But the state was conceived as unitary, as above society, and as legitimately penetrative of the entire territory and population under its jurisdiction.

It was this conception of the centralized, superimposed absolute state, embodied in the sovereign and the agencies of the "crown," that was the target of the pluralists of the late nineteenth and early twentieth centuries. Opposition to this all-powerful notion of state sovereignty came both from the Right and the Left. From the conservative side J. Neville

Figgis (1914) affirmed the autonomous rights of churches and neighborhood communities. From the Center and the Left Ernest Barker (1930), A. D. Lindsay (1929), and Harold Laski (1919) affirmed the autonomy of economic and professional groups and trade unions. The English theorists drew on German and French authorities—Otto Gierke (1900), Emile Durkheim (1950), and Leon Duguit (1919)—for authority for their antimonism. What is crucial for the argument of the contemporary statists is Francis Coker's (1934) unequivocal statement that

> Neither the pluralists nor their theoretical forerunners, however, really make the groups independent of the state. Gierke maintained that the state is sovereign—supreme in legal and moral right—where general interests require the exertion of organized social power for their maintenance. Paul Boncour regarded the state as the sole organ of national solidarity, with a duty to prevent any group from acting oppressively toward the public, other groups or its own members. Durkheim ascribed to the state the function of defining general policies, leaving to the several associations only the task of diversifying, under state supervision, the application of the policies according to the special requirements of the several associations. Figgis described the state as the *communitas communitatem* and assigned to it distinctive tasks and a superior authority as the chief agency of social adjustment and coordination. . . . Barker warns against carrying the recent pluralist trends too far; the state as the most embracing scheme of social life must be allowed to adjust the relations of associations to one another and to their members in order to preserve the equality of associations, and protect the individual from the possible tyranny of his own group. (p. 172)

Laski—the most extreme of the antimonists—goes far towards recommending a powerful state that would own monopolistic industries; regulate the rest of the economy; set basic standards for hours, wages, and working conditions; and fix prices for necessities. Coker concludes,

> It appears then that when the pluralists set forth their abstract theory they deny the sovereign power of the state or else characterize the power of something which is properly only ultimate and reserved; but when they devise the specific institutional arrangements to carry out their theory they assign to the state numerous tasks, in laying down general policies and seeing that they are observed, which obviously require, not an ultimate and reserved, but a very direct and constantly exercised power. . . . They would retain the state but deprive it of sovereignty. It appears, however, that they accomplish this compromise only in words. They allow the state to secure its funds through compulsory taxation, retain the whole traditional system of a compulsory allegiance applied to all members of the community and assign to this comprehensive and coercive association extensive duties in directing the economic and social life of the community. (1934, 173)

In the *International Encyclopedia of the Social Sciences* (1968) Henry Kariel gives a version of pluralist theory somewhat more supportive of the reductionist government-as-arena model. But here he is referring to the earlier formulations of the theorists of pluralism. He reports that by the 1930s "it appeared necessary to some of the pluralists to reintroduce what they had previously banished; a unified purpose above and beyond the will of a plurality of groups. . . . They had rejected such ideas as common good, community interests, and general will. Yet they found it scarcely possible to conceive of the political process without the purposeful, helpful hand of the state, especially as domestic group competition, and foreign threats endangered the viability of a pluralistic political order. Thus both Laski and Cole were finally driven to recognize needs more fundamental than a vibrant group life. They were to argue for both leadership to give expression to these needs and a state equipped to satisfy them" (p. 507). Kariel insists, however, that a latent integral pluralism persists in some unspecified part of mainstream political science.

The pluralist writings that are viewed as coming closest to the portraits presented by the statists are those of Arthur Bentley (1908, 1967), David Truman (1951), and Earl Latham (1952). Bentley's *Process of Government* received almost no attention for the several decades between its publication in 1908 and the appearance of Truman's book in 1951. In his introduction to the 1967 edition of Bentley's book, Peter Odegard remarks on the fact that Bentley provides little empirical data on interest group activity, despite the fact that pressure groups and the lobby were high salience issues at the turn of the century. His book was essentially theoretical; it came close to presenting a formal model, an analytic reduction of institutions to observable behavior with interest groups the all-powerful explanatory variable. Peter Odegard argues that had the book been more balanced as between modeling and empirical data and had the model been presented as a model rather than as an abstraction of reality, Bentley might not have been so long neglected by scholars. Thus the large U.S. pressure group literature that appeared in the 1920s to 1940s was not influenced by his work; and the pressure group literature of the 1950s and 1960s sometimes cited Bentley, but it did not adopt his group reductionist and behaviorist model. The empirical pressure group research of the first decades of the twentieth century originated in the muckraking tradition of the turn-of-the-century period.

David Truman, the leading group theorist of recent political science, utilized Bentley as an analytical model but devoted almost half of his trail-breaking book to the interactions of interest groups and governmental processes. Though governmental institutions are treated from a group perspective, there is clear recognition of their autonomous decision-making capacity. Thus in his chapter on the executive he points out that

> The importance of the executive branch, however, does not derive simply from its size or from the variety of its activities. It is of far greater significance that the operation of these activities necessitates choices among alternate lines of action, the exercise of discretion. . . . Both the newer and the expanded older functions of government create the necessity for a large measure of administrative discretion, and the tendency is toward the widening of such powers rather than toward their restriction. . . . The obligation to remain minimally accessible to all legitimate interests in the society can supply him with a measure of independence and a persuasive power that effectively supplements his formal authority. . . . Access to the governor or president is not sought by interest groups simply out of habit; the means of leadership in the hands of these officials are not negligible. The president's roles as chief executive, chief legislator, chief of state, and commander of the armed forces are real, not nominal. (1951, 396, 427).

It is not possible save through distortion to describe these several chapters depicting the interaction of groups and government, as simply reflecting the arena or the cash register model. Executives, congressmen, and judges have both authority and discretion.

In his article, "Political Group Analysis" in the *International Encyclopedia of the Social Sciences* (1968), Truman refers to this "referee" model of governmental processes as an oversimplification leading "to the error of treating a limiting case as if it were the norm. This obvious error occurs with great frequency in the literature, despite the fact that even our rough knowledge of the distribution of cases of the relation between interest group and government demonstrates that this oversimplification fails to square with the evidence concerning a vast range of instances" (pp. 243–44).

Earl Latham (1952) does present an integral group theory of U.S. government. He does this by characterizing governmental institutions as groups themselves; and argues the case for a group politics equilibrium model of the policy-making process. But he runs into problems with governmental agencies as groups. He finds that there is a unilateral, asymmetrical quality about government as groups. The policeman can stop traffic; drivers cannot stop the policeman. He gropes in an effort to accommodate the peculiar properties of officials. He rejects Krasner's metaphor of the pluralist state as a cash register more than 30 years before Krasner uses it: "In these adjustments of group interest, the legislature does not play the part of inert cash register, ringing up the additions and withdrawals of strength; it is not a mindless balance pointing to and marking the weight and distribution of power among the contending groups" (p. 391). Thus both houses of Congress are groups with senses of identity and with their own interests. Executives, legislators, and judges are "officials"; if they are members of groups, their groups are not of the same order as nonofficial groups. He writes, "The groups so privileged collectively make up the instrumentalities of the state, and such groups are distinguished from others only in their possession of the characteristics of officiality" (p. 389).

The reductionist model, while applicable to the work of Bentley and Latham, can only with difficulty be applied to that of Truman. And it completely fails to capture the empirical interest group research of the 1920s to 1960s, the very literature described as society-centered by the neostatists. In demonstrating that this literature operated with a very lively sense of the autonomy and discretion of government, both empirically and normatively, I shall cite a selection of Americanists—in particular Pendleton Herring; Elmer Schattschneider, V. O. Key, Jr.; Robert Dahl; and Bauer, Pool, and Dexter—and Harry Eckstein, Henry Ehrmann, and Joseph La Palombara among the Europeanists. On the question of societal reductionism more broadly in the political science literature of the 1960s and 1970s, I shall review briefly some of the U.S. and comparative government text literature of this period.

One of the most influential political science studies of the 1930s was Pendleton Herring's *Public Administration and the Public Interest* (1936). His principal question was how administrative organization in a democracy could resist the onslaught of particularistic groups and its own conservative propensities and contribute to the general welfare. In the light of the conflicts of the New Deal period he points out that the democratic state must somehow deal with the conflict between capital and labor. "It cannot afford to await the survival of the fittest. The exigencies of the age demand positive state activity in order that democracy itself may continue" (p. 379). He continues, "Unless we are to rest resigned with this situation upon the supposition that this is the inherent nature of the democratic state, there is need for promoting a purpose of the state over and above the purposes of the medley of interests that compose it" (p. 380).

On the autonomy of the state and the need for increasing this autonomy Herring writes, "The need of attempting to formulate an official program in the public interest by a responsible administrative agency arises from the experienced strength of minority groups in pressing their case by propaganda and organized agitation. . . . The offering of positive proposals by a responsible administration is then the first goal to seek. . . . The increased power of the president means, of course, an increase in the importance of the bureaucracy. . . . If the existence of the democratic regime rests upon the assumption that the state exists not for the welfare of any one class but for the benefit of the people as a whole, this great and growing bureaucracy must be guarded from domination by economic groups or social classes" (pp. 383–84). This whole concluding chapter of Pendleton Herring's work of the mid-1930s might well serve as a bible for the contemporary state autonomy movement, even to the point of the use of the same vocabulary.

Elmer Schattschneider, a leading authority on U.S. politics and pressure groups of the 1930s and 1940s, describes the U.S. presidency as the principal rallying point for the great public interest of the nation, the point at which the issues of public policy are discovered and exploited (1942).

V. O. Key, Jr.—the leading U.S. theorist of parties, elections, public opinion, and pressure groups—whose views are crucial for the statist critique of pluralism, has a long concluding chapter, "Pressure Groups and the General Welfare" in his text on political parties, first published in 1948 and appearing until well into the 1960s. He argues that public officials and political leaders "can frequently obtain acceptance of a policy more nearly reflecting the general interest by going over the heads of pressure group leaders to workers, businessmen, to farmers who have ordinarily a greater sense of community responsibility than do their hired men in the offices of pressure societies" (1952, 174). To illustrate the capacity of government to resist special interests, Key cites major statutes regulating the interests of powerful groups in the 1930s—among them the Public Utility Holding Company Act, and the Food, Drug, and Cosmetic Act—where powerful organized interests were present and actively lobbying against the legislation and where there were no organized groups representing the ordinary citizens whose interests were protected by these acts. These last two examples would come close to representing Nordlinger's type 1 cases of state autonomy.

The two classic political process studies of the 1960s—Dahl's *Who Governs?* (1961) and the Bauer, Pool, and Dexter *American Business and Public Policy* (1963)—both reported results reflecting the autonomy of governmental officials and governmental agencies. In their conclusions on the relative importance of "societal" and governmental agencies in the enactment of the Reciprocal Trade Agreements Act of 1954-55, Bauer, Pool, and Dexter point out that "a Republican administration"—normally most susceptible to the pressure of business interests for protection from foreign competition—". . . felt free to adopt as the most cherished item of its legislative program a Roosevelt measure which transferred tariff making from the legislative to the executive branch" (1963, 466), where, it should be pointed out, tariff making would be less exposed to group pressures. Indeed, their study of business interests and tariff policy in the post-World War II period records the attenuation of "societal" influences and the attainment of "state autonomy" with respect to this field of foreign economic policy. Dahl's study of redevelopment policy-making in New Haven in the 1960s has the well-deserved reputation of having moved the study of policy-making forward by demonstrating the strong relationship between political structure and process and the substance of public policy. Rather than being societally reductionist, Dahl's study was directed against the community power model of C. Wright Mills, Floyd Hunter, and others, which was reductionist, attributing generalized and dominant political influence to the upper business and professional interests. Dahl demonstrated that New Haven had a number of different policy processes that were triggered by different kinds of issues. In the case of redevelopment in New Haven, he shows that Mayor Lee initiated this policy long before there was any interest group or popular movement favoring urban renewal. Krasner (1984) acknowledges that this was the case, but insists that Dahl has reduced "state institutions" to "individuals and roles," failing to take into account the constraints of institutions. I will deal with this version of the reductionist thesis in the context of how statists define state autonomy.

In an authoritative work on governmental regulation in the United States, James Q. Wilson (1980) writes in his introduction, "Today the federal government is active with respect to virtually the full range of human affairs, much of its power is exercised, and many of its purposes are defined, by a large bureaucratic apparatus" (p. vii). In his concluding discussion of the origins of regulation he shows how neither a societal nor a statal "paradigm" would capture reality: "The Civil Aeronautics Board (CAB) was created to help business, but the Occupational Safety and Health Administration (OSHA) was formed over the objections of business. The laws administered by the Environmental Protection Agency (EPA) were enacted over the opposition of those segments of the economy to be regulated. . . . The Shipping Act that was to be administered by what later became known as the Federal Maritime Commission (FMC) was passed over the opposition of the shipowners whom it was to regulate, but with the support of shippers it was supposed to help. Though the Sherman Antitrust Act did not have to overcome well-organized business opposition, neither was the chief impetus for its passage the demand of business" (p. 364).

During the 1920s and 1930s, at a time when U.S. pressure group studies were already thriving, European scholars viewed pressure groups in reductionist and normative terms. The political scientists of the Left tended to reduce pressure groups to class phenomena. Conservative European political scientists viewed pressure groups as political pathologies. Only the state and the bureaucracy had legitimate power and were capable of acting in the general interest.

After World War II a number of empirical studies of interest groups in Britain, France, Germany, Italy, and other countries began to appear. For the purposes of our argument here it may be sufficient to review three exemplars of this literature: Harry Eckstein's study of the British Medical Association (1960), Henry Ehrmann's study of organized business in France (1957), and Joseph La Palombara's (1964) study of Italian interest groups.

In the introduction to his study of the British Medical Association Eckstein points out,

> Pressure is concentrated upon the executive in Britain, first because of the logic of cabinet government in a political system having two highly disciplined parties; such a system precludes any consistently successful exertion of influence through members of Parliament, or, less obviously perhaps, through the political parties. Secondly, pressure is focussed on the executive because the broad scope and technical character of contemporary social and economic policies has led to a considerable shift of functions to the bureaucracy. . . .
>
> "The predominance of skeletal legislation is not due solely to circumstance (e.g. technical legislation) but also reflects the power of the executive which likes to have a relatively free hand." (1960, 18, 20)

He points out that pressure groups are anxious not to get on the wrong side of the government because of the crucial powers that they enjoy over their affairs. Commenting on Bentley's one-sided interpretation of group influence, Eckstein observes, "The state in Britain today disposes directly of 40 percent of the national income, and that fact speaks for itself. We may regard political systems as amalgams of potential and actual pressure groups, groups which from a political standpoint are merely 'categoric' groups, and groups which have actually been drawn into politics, chiefly through the impact of public policies, either policies actually adopted or policies which are threatened. In short, we can usefully stand Bentley on his head to supplement Bentley right side up; if interaction among politically active groups produces policy, policy in turn creates politically active groups" (p. 27).

According to Eckstein the influence and effectiveness of pressure groups is substantially dependent on governmental policy and organization. His approach is neither societally reductionist nor statist reductionist. Like almost all of his pluralist colleagues he sees the policy process as a set of interactions between state and society.

Ehrmann's (1957) study of business groups in France emphasizes the tradition of central administrative power. He notes the importance of the nationalized industries under the Fourth Republic, the control of money and credit, exports, and convertability, governmental price fixing, and the like. He describes the transformations of the Fifth Republic as shifting power from the parties and parliament to the executive and bureaucracy. In language that would gratify the most "statist" of the neostatists Ehrmann catalogs the major triumphs of President de Gaulle in battles with organized interests:

> In regard to major decisions, e.g. the Algerian or the European policies, the liquidation of the African Community, or the preparations for an atomic striking force, the interested lobbies (some of them solidly organized and amply financed) were generally ignored by the Elysee and had difficulties in obtaining a hearing in the offices of the Prime Minister. . . . In two of his most resounding conflicts with the National Assembly the President of the Republic wanted it to be known that he denied to the deputies the exercise of rights granted to them by the wording of the constitution because parliament was in fact obeying the injunction of the agricultural lobby. Such affirmations of the regime's fundamental hostility towards intermediaries standing between 'le pouvoir' and 'le peuple' were designed to dramatize its intentionally authoritarian style. (1963, 278)

To similar effect La Palombara (1964) in his study of Italian interest groups has a whole chapter dealing with the factors that enable the Italian bureaucracy to resist the pressure of interest groups. Among these factors is the culture of the bureaucracy, the sense that the official has of the general or the public interest as he encounters the overtures, blandishments, and threats of pressure organizations: "The typical bureaucrat recognizes that he is involved in a highly politicized process and that the concept of public interest is one of the few meaningful weapons he can utilize in order to assert a bureaucratic decision that is not tied to narrow, particularistic interest group considerations" (p. 384). The socialization of the bureaucrat in the administrative organization also contributes to the capacity to resist the pressure of special interests. La Palombara concludes from his interviews of Italian officials that "when bureaucrats in large number define their role as requiring the objective application of the law, they generally do so with pride and with a sense of having to protect the bureaucracy—and therefore the nation—against the irrational and particularistic forces at large in the country" (p. 388).

The literature that we have reviewed on pluralist theory, group theory, and the activities of interest groups in the United States and Europe gives the statist polemic the best chance of being sustained. It is after all this literature that has been arguing the case for the influence of "societal" organizations in the making of public policy. Though this literature does by and large stress the importance of interest and pressure groups in policy-making, it clearly does not support the reductionist thesis. Autonomous government agencies are present and important throughout this literature. The pluralist "paradigm" is not the one-sided one of Skocpol, Krasner, and others but rather a two-directional one with the state influencing the society, as well as the society influencing the state.

If the statist polemicists cannot be sustained with respect to that literature most likely to support their thesis, how does it fare with respect to the more general political science literature of the pluralist era, for example in the U.S. and comparative government text books of the 1950s and 1960s? A few examples may make the point. The two leading U.S. textbooks of the 1940s to 1960s, written by William Anderson and the perennial (Frederick) Ogg and (P. Orman) Ray, give great stress to the powers of the U.S. presidency. Anderson speaks of the U.S. executive as "the government in a very real sense." He has "the power to formulate programs of action" (Anderson and Weidner 1953, 578). Ogg and Ray speak of the

U.S. president as "the most powerful elected executive in the world. He is at once the chief formulator of public policy as embodied in legislation, leader of a major political party boasting thousands of functionaries and millions of adherents, chief architect of U.S. foreign policy, . . . director of one of the most gigantic administrative machines ever created, . . . commander in chief of more than two million men in uniform," and so on (1959, 258).

Austin Ranney in a text of the 1960s describes the president as making a great deal of law: "He is generally regarded as our 'chief legislator,' . . . mainly because he has taken over most of the initiative in the nation's statute-making process" (1966, 442). In the 1963 edition of another leading text on U.S. government, Burns and Peltason's *Government by the People,* the authors also speak of the president as the chief legislator (pp. 424–25). Indeed, without exception, the texts of the pluralist era in U.S. political science refer to the president as the chief legislator and policymaker of the nation.

The texts in comparative government during these decades gave similar stress to the importance of the political executive. Carter and Herz (1972) speak of the political executive in democracies as laying down "the essential lines of policy in every field of domestic and foreign affairs; he must coordinate the innumerable agencies of what, in most modern countries, has become the biggest business establishment"—"the executive branch of government. . . . he must deal with a vast variety of interest groups and be able to resist their often powerful pressures" (p. 152). And in the Samuel Beer and Adam Ulam text edition of 1964, the chapter on Britain describes the British Cabinet as the very center of power and the higher civil service as the initiator and drafter of almost all legislation (p. 103).

The conventional wisdom of the political science literature of this period emphasized the importance not only of the political executive but of the bureaucracy. Thus Robert Putnam exclaimed, "Public bureaucracies, staffed largely by permanent civil servants, are responsible for the vast majority of policy initiatives taken by governments. Discretion not merely for deciding individual cases, but for crafting the content of most legislation has passed from the legislature to the executive. Bureaucrats, monopolizing as they do much of the available information of shortcomings of existing policies, as well as much of technical expertise necessary to design practical alternatives, have gained a predominant influence over the evolution of the agenda for decisions" (1973, 17).

It might be expected that the political sociologists would be more likely to manifest the society-centeredness that the statists have attributed to the pluralists and the structural functionalists. Since sociologists specialize in social structure and processes, it might be expected that they would incline toward explaining the state and politics in terms of society. In a series of papers on the scope and content of political sociology going back to the 1950s, S. M. Lipset and William Schneider trace the history of this debate from the state-centeredness of the Hegelians to the society-centeredness of Proudhon and Marx. In Max Weber and the later political sociologists the causal arrows go both ways, and both concepts —state and society—are disaggregated into their component parts. Lipset and Schneider argue, "Political sociology can be defined as the study of the interrelationship between society and polity, between social structures and political institutions. It is important to note that this definition does not assign causal priority to society over polity; political sociology is not solely the study of the social factors that condition the political order. Indeed political institutions are themselves social structures, and hence are often the independent (that is) causal factors that affect nonpolitical social structure" (1973, 400–401).

A Note on Marxism and Structural Functionalism

The evidence on the pluralist literature, both theoretical and empirical, clearly cannot support the neostatist critique of society-centeredness. But the neostatists also included structural functionalism and Marxism in their polemic against society-centeredness. With respect to Marxism, they are without question correct. Skocpol argues that "at the theoretical level, virtually all neo-Marxist writers on the state have retained deeply imbedded society-centered assumptions, not allowing themselves to doubt that, at base, states are inherently shaped by classes or class struggles and function to preserve and expand modes of production" (Evans, Rueschemeyer, and Skocpol 1985, 5). Martin Carnoy's review (1984) of neo-Marxist theories of the state—from Gramsci to Althusser to Poulantzas and Offe—by and large supports Skocpol's (1982) summary.

Indeed, David Easton (1981) characterizes the entire "return-to-the-state" movement as arising out of a contemporary revival of Marxism. He speaks of three Marxist "comings." The first coming occurred at the time of the origins of the Marxist movement, which touched the United States only superficially in the 1880s and 1890s. The second coming occurred in the great depression of the 1930s. This impingement affected U.S. academic life and political science more deeply. The whole, now older, generation of political science was affected by this second wave and by the disillusionments of the late 1930s and 1940s. The third coming of Marxism had taken the form of this neostatist movement—more complex than the first two visitations, but presenting some of the same difficulties.

The polemic against structural functionalism is the least-developed theme in the neostatist movement. It is said to be societally reductionist, but no attention is given to the "structural" part of structural functionalism, which after all deals with state agencies and institutions. "Functionalism" developed out of the realist recognition that legal or customary norms defining the powers of the various institutions usually failed to capture their performance. Often they were quite misleading. Hence the properties of institutions were not taken as normatively given but as something to be researched empirically. And particularly if one wanted to compare political systems across cultures, one had to examine functions as well as normative and legal structures. Structural functionalism is as old in some ways as political science itself. Separation of powers—a structural-functional theory—has been a central theme of political theory at least since Montesquieu.

Neostatist Definitions of the State

I promised to analyze the statist movement on three levels of validity and utility. The first of these was the strong statist position that pluralism–structural functionalism and Marxism were societally reductionist, that they did not attribute autonomy to the state, and hence were inadequate approaches to political explanation. Our analysis of the evidence suggests that this characterization of pluralism both in theory and in empirical research is incorrect. Overwhelmingly, the pluralist literature has been shown to be one in which governmental autonomy is recognized; in which the explanatory logic goes in both directions, from society to the state and from the state to the society. Neostatism seems to be a polemic internal to Marxism, since this intellectual movement has in the past decades operated from a social class reductionist set of assumptions.

If the critique by the neostatists of the mainstream literature of political science has not been supported by evidence, it may still be that the statist approach is significantly superior to pluralism-structural functionalism as an approach to explanation, that it specifies the elements and variables of analysis better, that its causal logic is more powerful, and the like. The writers in this genre may have been careless and mistaken in their search of the literature, but their approach to research design may be superior.

The shift from institutionalism to realism in the political science of the late nineteenth and early twentieth centuries was enormously productive. It gave birth to the work of Vilfredo Pareto, Max Weber, Emile Durkheim, Woodrow Wilson, James Bryce, Moissaye Ostrogorski, Roberto Michels, Graham Wallas, George Catlin, Charles Merriam, Harold Lasswell, and Harold Gosnell, among others; to the large empirical literature on political parties and pressure groups; and to realistic studies of governmental institutions and processes. This movement of "realism" assumed that by "unpacking" and disaggregating general and diffuse concepts it was possible to reach a firmer grasp of the reality scholars were seeking to understand, interpret, and explain. We understand liberty, justice, equality, obligation better by spelling out their varieties and their component parts. Similarly, as a part of this movement of realism, legal-institutional concepts such as the state were disaggregated. One spoke of government or of the political system and its various legal-institutional components—executives, bureaucracies, legislatures, and courts—and of agencies and institutions of a paralegal and nonlegal sort such as political parties, interest groups, media of communication, and the like.

The neostatist movement wants to reverse this trend and return to large and relatively loosely defined concepts such as the state and society. They write about "strong and weak states" and "strong and weak societies," but it is not clear what they mean by strength and weakness. So many dimensions are conflated here that they cannot expect this approach to research to be taken seriously. Indeed, it is an irony of the statist movement that this central concept either is not clearly specified or, when it is specified, is very like the definitions of *government, political system,* and other terms denoting the totality of political phenomena employed in the mainstream literature.

Theda Skocpol approximates a definition of the state in her earlier work on revolutions, where she sets aside the neo-Marxist efforts of Miliband, Poulantzas, Anderson, Therborn, and Offe because they fall short of acknowledging the possibility of state autonomy. She writes,

> We can make sense of social revolutionary transformations only if we take the state seriously as a macro-structure. The state properly conceived is no mere arena in which socio-economic struggles are fought out. It is rather a set of administrative, policing, and military organizations headed, and more or less coordinated by, an executive authority. Any state first and fundamentally extracts resources from society and deploys these resources to create and support coercive and administrative organizations. Of course, these basic state organizations are built up and must operate within the context of class-divided socio-economic relations as well as within the context of national and international dynamics. Moreover, coercive and administrative organizations are only parts of overall political systems. These systems also may contain institutions through which social interests are represented in state policy-making as well as institutions through which nonstate actors are mobilized to participate in policy implementation. Nevertheless, the administrative and coercive organizations are the basis of state power as such." (1979, 29)

Skocpol is struggling here to free herself from the class determinism of Marxism. She even adopts the system metaphor to accommodate important aspects of politics not easily reducible to coercive and administrative organizations. But in her later work she comes back to a concept the limits and content of which are unspecified. Thus in her more recent formulation (Evans, Rueschemeyer, and Skocpol 1985) she skirts the question of defining the state. What she does after critiquing the Marxist and pluralist-structural functionalist approaches to the state, is to cite favorably more "state-centered" types of research, such as the work of Nettl, Stepan, and others. For more remote ancestors of the statist approach she cites Max Weber and Otto Hintze. Because her approach is through illustration rather than specification, it is difficult to distinguish the extent to which and the ways in which her state-centered approach would differ from the treatment of the state and government in the work of Herring, V. O. Key, Eckstein, Ehrmann, the text writers in U.S. and comparative government. It is also impossible to determine how it differs from the treatment in structural functionalism of executives, bureaucracies, courts, legislatures, parties, pressure groups; or how it compares with the enormous literature of the 1960s and 1970s that could not properly be subsumed under either of these approaches.

In a later chapter in the SSRC volume (Evans, Rueschemeyer, and Skocpol 1985), in collaboration with Margaret Weir, Skocpol presents a number of diagrams of the policy process. She rejects two that begin the policy process with "socially rooted demands" or "authoritative intellectual developments" in favor of the third one that presents the starting point of policy explanation as "state structures and policy legacies," and in which the activities of politicians, and officials, and demands of social groups are treated as intervening variables. She is clearly insisting here on a "state-centered" explanation of the policy process. I suggest that the important contrast here is between the state-society interaction model of mainstream political science and this state-centered one.

In chapter 2 of this volume Dietrich Rueschemeyer and Peter Evans adopt the Weberian definition of the state as the agency that seeks and maintains a monopoly of legitimate violence in a given territory. This is not substantially different from mainstream efforts to define government and the political system (e.g., Almond and Coleman 1960, 6, 7). And if one reads the fine print in the statist literature it is clear that their definitions are quite similar to mainstream definitions. Leaving aside the neo-Marxist vocabulary, the conclusions of Rueschemeyer and Evans as to the complexity and diversity of state phenomena comparatively considered are really quite eclectic and mainstream: "We recognize that across a range of historical circumstances—in ways that vary substantially—the state tends to be an expression of pacts of domination, to act coherently as a corporate unit, to become an arena of social conflict, and to present itself as the guardian of universal interests" (Evans, Rueschemeyer, and Skocpol 1985, 48).

Stephen Skowronek's *Building a New American State* (1984), often represented as the exemplar of the new statist paradigm, does not offer a definition of the state. The narrative of this important contribution to U.S. administrative and political history, is carried not by the "state" but by the "government," the "administration," or the "bureaucracy" or by particular agencies of government like Congress, courts, and political parties.

Stephen Krasner goes a great deal further in specifying the essence of the statist approach. He attributes five characteristics to the recent statist literature "that distinguish it from orientations associated with the behavioral revolution" (1984, 224). Echoing a criticism of Theda Skocpol, Krasner states that the statist approach views politics more in terms of rule and control than of allocation. Krasner does not seem to be aware here of the political science literature on regulatory policy and agencies, on the emphasis in the structural-functional literature on extractive and regulative, as well as the distributive, aspects of politics. The early and influential theoretical work of Lowi (1964) whose threefold classification of types of public policy—the regulative, distributive, and redistributive—has had such great influence in empirical political science research, is also unmentioned by Krasner.

A second characteristic of the literature is the argument that the state is an actor in its own right. "It cannot be understood as a reflection of societal characteristics or preferences" (p. 225). Here Krasner is simply repeating the myth of the societal reductionism of pluralism.

Krasner's third point is that statist studies place more emphasis on institutional constraints on individual behavior. Statists, according to Krasner, accord only a very small scope of discretion and initiative at the individual level: "Actors in the political system, whether individuals or groups, are bound within these structures, which limit, even determine, their conceptions of their own interest and their political resources" (p. 225). On this point Krasner is probably correct. Most mainstream political scientists would draw back from a thoroughgoing determinist position.

Krasner's fourth contrast has to do with viewing political phenomena in a longitudinal and historical perspective. He makes much of the tree model of historical choice points, the argument that a particular choice tends to preclude other options. This decision tree model is, of course, a contribution from behavioral social science. The importance of the longitudinal, historical dimension has long been recognized by behavioral political science, in public opinion and election studies, in the political psychobiographical literature, and in structural-functional analysis. More recently, behaviorist students of the Congress such as Polsby (1968), Lawrence Dodd (1986a, 1986b), and Cooper and Brady (1981) have exploited the possibilities of the historical approach.

Krasner's fifth and final point is that behavioral political science tends to view political phenomena in equilibrium, process, and compatibility terms and fails to appreciate the importance of conflict and stress. This is difficult to square with the history of pluralism. Pluralism as a normative theory was born out of the struggle of economic, religious, and ethnic groups against the overpowering sovereign state, as well as the effort to conquer the state in order to use its coercive powers to benefit the interests of exploited groups and classes. The pressure group struggle described by Herring, Odegard, Schattschneider, Key and others, hardly took the form of a smoothly purring machine. Political culture research, consociational theory, corporatist theory, and development and modernization theory all treat conflict as central themes.

Thus with the exception of determinism in the structural sense, Krasner has listed a set of properties that do not demarcate statist studies from the main body of political science literature.

Some Concluding Reflections

Thus on two of the three grounds named, the statist movement is in error. Its attribution of societal reductionism to the pluralist-structural functionalist approaches is not supported by the preponderance of evidence. Its assertion that a statist approach is both different from prevailing views in mainstream political science research and is a distinctly improved approach to political explanation also fails to convince. Either it is diffuse or allusive in its formulation of what statism is or its definitions are indistinguishable from "behavioral" or structural-functional definitions of things political. More telling, perhaps, is the fact that in pressing their critique of older political science approaches to political explanation, the statists have fallen into the trap of neglecting nonstatal variables, such as political parties, interest groups, the media of communication, and the like. From this point of view it is a distinctly inferior model of political explanation to the models they criticize.

If statism fails in its polemic against pluralist reductionism and if its positive formulations do not convince, there still is a third ground on which to give it positive marks—the "heuristic" ground. It is surely the case in the history of scholarship that "stirring things up," even in the absence of substantive merit in a particular intellectual initiative, often proves to have been useful in the net and balance. Scholarly disciplines have a way of settling into ruts, becoming repetitive and endlessly replicatory or concerned with trifling modification and emendation of argument. A scholarly tantrum may have the effect of stirring the senses and alerting the mind to new possibilities.

Aside from generally capturing attention and provoking semantic adaptations on the part of many scholars, it may be argued that statism has drawn attention to institutional and particularly administrative history as a focus for political science research. This is all to the good. But if this, indeed, is the positive achievement of this intellectual episode, then one has to question why the literature of institutional and administrative history was not searched. Why are there no references to the four volumes of L. D. White's administrative history of the United States: *The Federalists* (1956), *The Jeffersonians* (1951), *The Jacksonians* (1954), *The Republican Era, 1868-1901* (1958)? Why no reference to White's *Trends in Public Administration* (1933)? More recently, why no reference to La Palombara's *Bureaucracy and Political Development* (1963), Ezra Suleiman's *Elites in French Society* (1978), John Armstrong's *European Administrative Elite* (1973), or Robert Putnam's *Comparative Study of Political Elites* (1976)?

If there is something basically at fault with this literature—and I am sure that the statists, coming from their primarily neo-Marxist and international relationist backgrounds have something novel to contribute to the tradition of institutional history—it is still a fundamental rule of scholarship that one searches the literature before venturing to improve on it. Nevertheless, on this quite limited ground it may be argued that the statist movement has performed a constructive role.

But at what exorbitant and unnecessary cost! A generation of young scholars has been encouraged to reject much of its scholarly ancestry with little more than a paradigmatic farewell. And they have been urged to adopt ambiguous phraseology in the place of a hard-won tradition of operational rigor.

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THE RETURN TO THE STATE: CRITIQUES

This symposium features three critiques of Gabriel Almond's argument—by Eric A. Nordlinger of Brown University, Theodore J. Lowi of Cornell University, and Sergio Fabbrini of the University of Trento.

"The Return to the State" is almost entirely devoted to a negation of statist claims for distinctiveness. It is executed by highlighting the state-specific components of pluralist and other "mainstream" scholars, and by minimizing the putatively special contributions of the statists. There is something to be said on both counts; but Almond goes too far, much too far. And while Krasner's statist perspective is not fully distinctive, it is possible to delineate one that is considerably more so.

Inflating our Statist "Ancestry"

How are we to know whether pluralism and its offshoots "typically," even "overwhelmingly," accord the state "substantial" autonomy? Almond refers to several reviews of the literature and a panoply of passages within it that deal with the state's autonomy. But their meaning and meaningfulness must be accurately and fully interpreted, beginning with the identification of the literature's central questions, answers, and assumptions. There is no disagreement about the overarching question that informs the work of most "mainstream" (and many Marxist) writers: how to account for the authoritative, highly consequential actions of the state, its public policies broadly conceived.

Almond does not stand back from his citations to interpret the thrust of pluralism's answers, but at one point he inadvertently indicates that they are decidedly society-centered—a critical point at that, given the "classic" status of *Who Governs?* (Dahl 1961): "Dahl's study was *directed* against the community power model of C. Wright Mills, Floyd Hunter, and others, which *was* [societally] reductionist, attributing generalized and dominant political influence to the upper business and professional interests" (italics added). According to Dahl, the political and politically fungible resources of societal actors are by no means limited to wealth, control over poverty, and social status. Other effective resources include numbers, votes, organization, information, expertise, a positive public image, access to the mass media, and nonfinancial kinds of economic leverage. Nor do they cumulate in the same hands; their distribution takes the form of "dispersed inequalities." Dahl's primary answer to the question, Who governs?, is thus: Just about everyone. "Virtually no one, and certainly no group of more than a few individuals, is entirely lacking in some influence resources" (p. 228). When set against the elitist depiction of society's political contours Dahl's answer is thus no less "societally reductionist." The working assumptions are the same: public officials are dependent upon societal support and constrained by the politically best-endowed groups.

The point can be made more broadly. Pluralist, neopluralist, and elitist works derive their normative, empirical, and analytic significance from the plausibility of the societal constraint assumptions, which concomitantly makes it all the more important to identify the private actors with the greatest policy influence. Differing interpretations of the relative effectiveness and distribution of private resources produce alternative portrayals of the effective demand groups, and thus of the extent to which political stratification is egalitarian and governments respond to majority interests. It is these issues—not any having to do with the barely discernible autonomy of the state—that are at the crux of the long-standing debate in which the divergent positions are represented by Dahl and the elitists, with the neopluralists (Lowi and McConnell) situated between them. Almond does not mention McConnell's (1967) important society-centered book or the latest major study to make the case for the exceptionally narrow distribution of effective political resources, Lindblom's *Politics and Markets* (1977). That the primary impact of this analytically and geographically wide-ranging work comes from the argument for "the privileged position of business" in polyarchies underscores the general point. And it is only once, and then ambiguously, that Lindblom speaks to the state's autonomy: "Caught in a potential crossfire between privileged controls and polyarchal controls . . . [government officials seek] to remove from politics those highly divisive issues on which businessmen would be loath to yield" (p. 205).

The societal constraint assumptions of pluralism and its offshoots are readily apparent in the issue typologies that have been most widely applauded and applied. Almond even directs us to them by criticizing Krasner for ignoring a typology (Lowi's) that "has had such great influence in empirical political science research." The same could have been said about the comparable framework developed by Wilson (1973), who also evokes Almond's appreciation. Analytically, both of them are firmly, almost entirely, anchored in society. Wilson's fourfold classification builds upon the extent to which policy issues entail a narrow or broad incidence of costs and benefits for private actors (e.g., those involving distributed benefits and concentrated costs). Lowi's typology also derives from the issues' significance for societal actors. Are the issues generalized or narrow, aggregative or disaggregative, related or unrelated as they bear upon private interests? Distributive issues involve individuals and firms, regulatory issues bring out more inclusive groups, and redistributive ones are pursued by social classes and peak associations. The common premise, then, is clearly that public officials are regularly responsive to societal demands, pressures, and sanctions. Why else elucidate the ways in which issue differences affect different societal actors and their relative policy influence?

Policy-making studies might well be expected to highlight the import of the officials who formulate, adopt, and implement public policy. Heclo (1974) sets out the explanations that have consistently been used to account for the formation of social policies: "socioeconomic variables," "the electoral process and party competition," interest group power," and "the internal workings of government itself" (pp. 6–9). It is of some significance that only one of them deals with the state and that the "most pervasive tradition identifies [competitive electoral bidding] as central." It is of greater significance that neither the few cited studies nor Heclo's own analysis includes instances or explanations of officials acting upon their policy preferences when these diverge from those of the politically best-endowed groups. Moreover, variables that could be used in both a state- and society-centered manner are exclusively cast in terms of the latter. Changes in a country's gross national product, for example, might explicate the state's policy preferences; yet it is only their effect upon the private actors' demands for social policy and their willingness to bear the costs that are deemed relevant. Roughly comparable statements apply to Cameron's (1978) synthesis of the five explanations for the expansion of the industrialized countries' public economies since 1945. Only one of them—the "institutional explanation," as it accounts for the officials' preferences for greater public expenditures—is exclusively state-centered. Only part of another—the officials' opportunities for creating a "fiscal illusion," in the "fiscal explanation"—says anything about *how* the state can act upon its preferences when these diverge from society's.

Despite their society-centered thrust, pluralist and related works may yet provide evidence and hypotheses that underscore the state's autonomy. Almond points to a veritable slew of writings that appear to do so. Yet only Dahl's urban renewal case shows public officials to be acting autonomously after having generated societal support; and only Bauer, Pool, and Dexter's analysis of trade legislation constitutes an instance of autonomous action in the absence of societal support. Nearly all the other references to the state fall into one (most into more than one) of four categories.

First, there are the references that are off the mark, even inadvertently underscoring the impact of societal pressures. Several pluralists are said to conceive of the state as the bearer of the public interest. But this is purely a normative claim when left at that. We wonder whether the state succeeded in transforming these preferences into public policy. And in Laski, Cole, Herring, and Schattschneider the public interest becomes a clarion call, an appeal, just because the state could not overcome societal constraints. How else interpret such phrases as, The state "must be guarded from domination by economic groups or social classes," "The exigencies of the age demand," There is a "need for," The "first goal" is, and "They were to argue for"—a state that can transcend private pressures so as to enact a positive welfare version of the public interest?

Second, there are the explanations for the officials' ability to turn their nominal powers into decisional power. The autonomy-enhancing conditions include the sizable portion of British gross national product that the state "disposes [of] directly," the French state's nationalized industries and "control of money and credit, exports and convertability," the Italian bureaucracy's "concept of the public interest, [which is] one of the few meaningful weapons" for fending off interest groups, the U.S. president's "persuasive power" and position as "commander in chief of more than two million men in uniform," and the special knowledge of most bureaucrats that allows them to shape "the agenda for decisions." These capacities and opportunities may well enhance the state's autonomy, but it is not shown that they are sufficient to counteract, neutralize, or circumvent societal constraints. In fact, the larger the number of policy levers and financial resources in the hands of public officials, the greater the societal demands and pressures brought to bear upon the state and the greater its dependence upon societal cooperation for the successful implementation of public policies. Except for the Bauer, Pool, and Dexter study, there is no mention of a single autonomy-enhancing condition that applies to the legislature. And how is it possible to ignore the U.S. state's most remarked-upon feature, the division and dispersal of power and powers as this is said to explain its minimal autonomy? According to one "influential" pluralist, the U.S. state was, and continues to be, divided so that the parts "would be captured by different interests" (Polsby 1971, 140–41).

Third, there are the partial statements, intimating that officials have turned their preferences into policy, without, however, indicating that they have actually done so. To be "responsible for the vast majority of policy initiatives," to "have the power to formulate programs of action," and to be the "chief legislator" is not necessarily to be acting on one's own preferences, as opposed to acting in order to maintain or acquire societal support. And if officials act on their own preferences, it is not shown that the policymaking efforts succeeded. Initiatives have been withdrawn and drastically compromised due to the intervention and mobilization of private actors, and they have been blocked by other officials who were (more) susceptible to societal pressures. Based upon some selective quotes, we are told that David Truman's "executives, congressmen, and judges have both authority and discretion." Leaving judges aside, where is the evidence of their autonomy? It may be expecting too much of a literature review to cite not just chapter summaries but also empirical verses. But how could Almond ignore the opportunity of a focused "polemic," given what was said (twice) in a statist study about the major work of "the leading group theorist of recent political science"? "Not one of the dozens of state units in Truman's compendious study adopted legislation or regulations that were opposed by the weightiest, or even the potentially strongest, of the interested groups" (Nordlinger 1981, 45, 151).

Lastly, there are a few studies that depict officials translating their own preferences into public policy. Yet even they are incomplete—and thus possibly less than telling—because they fail to indicate whether the state acted with or without the support of the politically best-endowed groups. Whether the latter's preferences coincide with, or diverge from, the state's is a critical issue. Acting on one's own preferences under conditions of divergence is obviously far more problematic, so much so that Krasner (1978, 55–57) and Skocpol (1979, 24–31) define autonomy exclusively in these terms. How then does one interpret the statement quoted from Key that public officials "can frequently obtain acceptance of a policy more nearly reflecting the general interest by going over the heads of pressure group leaders to workers, to businessmen, to farmers who have ordinarily a greater sense of community responsibility than do their hired men in the offices of pressure societies"? If group members control greater resources than their leaders, if they constitute the more effective demand groups, the officials' autonomy becomes less than impressive. The studies in Wilson's edited volume establish that regulatory policies were enacted over the opposition of particular business interests (e.g., industrial polluters, shipowners). But it is not said whether their political clout was greater than that of the much larger groups who variously welcomed, demanded, and worked for the enactment of regulatory policies. If not, these are instances of what Almond refers to as "the lowest level of state autonomy."

As for conclusions, this statement now speaks for itself: "Nordlinger fails to—and indeed cannot—document his characterization of liberal scholarship as societally reductionist." When taken as a whole, Almond's diverse literatures do take state autonomy as somewhat problematic, and—absent emphases and analytic development—there are some disparate accounts of the state's occasional autonomy. But that much has not been denied by the statists. Nor do Stepan and my society-centered characterizations of earlier writings refer to all the different current's in Almond's "mainstream." If it is thought that Almond's case could be made more persuasively, it should be noted that he has seized just about every opportunity to make it. Among important pluralist and related works he only missed Lowi's (1969) "interest group liberalism." Here the "most interested and best organized" groups are not only the most effective private actors to constrain the state, as in McConnell 1967, but they are also the preferred groups, those whose enfranchisement and demands were purposefully promoted by public officials in the 1960s (but not in the 1930s) for pragmatic *and* normative reasons—in the belief that these groups represent all significant societal interests (p. 71).

Statism Clarified

The assessment of the statists begins with some excessive and cramped commentary, moves onto reasonably firm literal ground, and then fails insofar as it hinges upon an inflated reading of statist elements into earlier writings.

With regard to conceptualization, several strictures about the state and its "strength" being defined in a conflated, underspecified, and overly encompassing manner are warranted. But beyond that Almond engages in excess. It is one thing to note Skocpol's inadequate definition of the state. She could also have been criticized for other vague formulations and central conceptual gaps, for the largely illustrative, underdeveloped (not just exaggerated) depiction of a "paradigmatic reorientation," and for her all-too-quick resort (for a statist) to societal explanations before exhausting state-specific possibilities. But it is quite another to dismiss and to conflate inaccurately Skocpol's concern with class-based power disparities with Marxism, impugn her putative "struggle" with it, and then criticize the resulting definition of the state for not being distinctive.

This last comment is also made about definitions of the state that do pass conceptual muster. Yet no statist has claimed definitional singularity; and there is no reason for definitions—as opposed to the questions and answers that they delimit—to be distinctive. Almond must be aware of the good "positivist" reasons (maximizing reliability, validity, replication, and cumulativeness) for not taking this path. Some conceptualization of "strong and weak states" do have their problems. But is it therefore reasonable to conclude that "this approach to research [cannot] be taken seriously"? Without mentioning its strong and weak state labels, Almond characterized my typology as having "interesting research possibilities." And not only does Krasner's (1978, 55-61) offer at least as many interesting possibilities, it is the centerpiece of an important, analytically developed, well-researched statist study that is not even mentioned.

The argument that statism is not at all distinctive then focuses upon the five characteristics that according to Krasner "distinguish it from orientations associated with the behavioral revolution." One is acknowledged as singular, after an egregiously cramped reading: a clause ("even determine")—in a sentence that delimits its scope, in a paragraph that makes its meaning fully apparent, in an article that does not refer to any works that fit with Almond's reading—is used to turn Krasner's institutionalism into a "thoroughgoing determinist position." This literal excess is also inexplicable. Almond could easily, and presumably would, have made the same point about institutionalism that he does about three other statist orientations—that it is not distinctive.

On a cramped interpretation of Krasner's tersely worded claim, Almond would be correct. Statist concerns with institutional explanations, problems of rule, control, and order, a polity's disjunctures, stresses, and struggles over allocational and procedural issues, and the past as it patterns the institutional present—these have all been analytically developed and investigated within the "mainstream," several of them in one of Almond's important contributions (1973). Indeed, the case could have been strengthened by noting Krasner's characterization of statism "as being concerned with two central issues: the extent of state autonomy and the degree of congruity between the state and its environment" (1984, 224). For the latter dilutes the claim of distinctiveness by implicitly assigning roughly equal importance to the societal environment, as in Skowronek's analysis (hardly a "narrative") of industrialization's powerful challenges to the localized, nationally fragmented U.S. state and its consequent adaptation.

On a more generous reading that pays attention to the qualifiers (*emphasis*, *more*, and *emphasize)*, Krasner is obviously not claiming *de novo* status for four statist characteristics. He is saying—and with considerable warrant—that it is only recently that they have been consistently and tenaciously focused upon the state itself. The fifth distinguishing feature is critical. Krasner says that the state's autonomy is one of the two "central issues," with Almond presumably agreeing, since half his article serves as the underpinning for this terse assertion: "Here Krasner is simply repeating the myth of the societal reductionism of pluralism." The response can be equally terse: for the most part, it is hardly a myth.

The statist perspective is thus—Shall we say discernibly, substantially, or decidedly?—distinctive. And it could be made more so by ordering, delimiting, and then expanding upon Krasner's guidelines. Focusing upon the state as an independent actor turns the most distinctive, analytically significant, and intrinsically important assertion into the perspective's centerpiece. As an independent variable, the focus is upon the state's recurring directives and institutional features as they impact upon its autonomy and society. Krasner's other three characteristics are incorporated insofar as they help explicate past and current variations in the autonomy and impact of the state.

Before delineating this perspective it is worthwhile commenting upon the relevance of the most important paradigmatic issue that the discipline is currently "working through," that of the rational actor model's explanatory power. As part and parcel of their institutional focus, Skocpol rejects and Krasner sharply downplays analyses rooted in atomistic individual actors. And for whatever reasons, not one acknowledged statist has worked with positive choice theory. Yet in principle and future practice there is no reason why a distinctive statist perspective cannot be derived from it, at least in part. Just as Krasner can claim singular status for statist studies insofar as their institutionalism is focused upon the state and its autonomy, a reliance upon positive choice theory would not necessarily detract from statism's distinctiveness. Leaving aside the costs and benefits of the deductive rigor to be derived from an exclusive concern with narrow self-interests, rational actor analysis and cultural and institutional approaches are not incompatible, inasmuch as they deal with different levels of analysis. Pluralist and statist studies rely heavily upon self-interested motivations. And rational actor analyses can illuminate the state's institutional contours as one kind of collective outcome, as in Mayhew's (1974) interpretation of the consequences that flow from Congress's "electoral connection."

There is, however, a problem. Its presence is implicitly acknowledged in Almond's article, which does not allude to a single positive choice writer (they are surely in the "mainstream") who has assumed or discovered an autonomous official. Beginning with Downs (1957), when these scholars have dealt with state-society relations, they have consistently invoked the societal constraint assumptions. It is not just U.S. congressmen but also the officials of democratic and authoritarian regimes in Latin America (Ames 1987) and Africa (Bates 1987), whose rational "survival" interests prompt them to act as entrepreneurs in acquiring and maintaining societal support, as when markets are replaced by marketing boards and other politicized agencies so that officials can distribute favors to the clients upon whom they depend politically. With Almond's article signaling that the autonomy of the state has become (or saying that it always was) part of the conventional wisdom, positive choice theory has good warrant to investigate more than just the officials' reelection and survival dependencies. The behavioral and institutional consequences of other interests, beginning with the officials' search for autonomy, can also be rigorously spelled out. So, too, can the outcomes that derive from the interests of officials who enjoy considerable security of tenure and autonomy vis-à-vis private actors, as with Niskanen's (1971) budget maximizing bureaucrats and their relations with legislative committees. Given the consistent claims of public choice writers that economic slowdowns and similar "irrationalities" are attributable to the interventionist policies adopted by politically dependent officials, it would be worth asking how autonomy-seeking and autonomously positioned officials affect economic performance. That is a question that would certainly fit into a statist perspective.

The State As an Independent Actor and an Independent Variable

The core of this statist perspective features (1) public officials' forming their own policy preferences, the extent to which they do so being explained by the state's internal variations, and the preferences being distinctive vis-à-vis society's; (2) the state acting on its preferences despite their divergence from those of the most "powerful" private actors, with its internal characteristics patterning the frequency and strategy by which it does so, in the past and present; (3) the state's recurring activities and institutional contours' impacting upon society and the efforts of private actors to constrain the state; (4) and the state enjoying analytical priority with its autonomy determinants being used to identify the most important among the societal actors and variables that impinge upon it.

As an independent actor, the state's agenda of preferences is very much its own. Public officials are minimally malleable—susceptible and receptive to societal preferences—in forming and altering their own. Private interests are treated as "data" to be sifted, interpreted, and evaluated according to the officials' own lights, interests, and priorities. Socioeconomic ills are diagnosed similarly. As an independent variable, it is the state's boundedness, cohesiveness, and differentiation that minimizes its malleability.

A well-bounded state features careerists who remain within its precincts and promotions that are at least partly based upon length of service. Having a bevy of currently relevant experiences and memories to draw upon, identifying closely with their vocation, and being well socialized into its mores, the careerists' preferences are not apt to be shaped by society's. The officials of a cohesive state hold substantially similar preferences and agree upon the decision rules for resolving disputes. Their chief reference groups are thus other officials whom they turn to for information, cues, and guidance when considering options in the formulation, adoption, and implementation of public policies. In a differentiated—corpuscular and specialized—state the many separate units generate their own preferences, and the officials' narrow responsibilities make for policy expertise and subjectively experienced competence.

There is neither need for, nor a disposition toward, "private tutoring."

It is not just that the autonomous state's policy preferences are its own, they are also decidedly distinctive. In their substance and underpinnings they do not regularly coincide with those of any larger or smaller societal associations, groups, strata, ethnic segments, or regions. What public officials do, whom they interact with, where they sit, and what they see and know help generate the singular interests, values, and beliefs underlying their preferences.

Officials attach process meanings and significance—the maintenance of routines and practices, coordination imperatives, convenience, and predictability—to the consideration, formulation, adoption, and implementation of public policies. Their preferences are shaped by organizational needs and rivalries over policy turf, budgets, internal autonomy, and prestige. And, all-too-often forgotten, governing is also an intellectual exercise. It involves distinctive policy frameworks, decisional criteria, and information focused upon the policy options' ramifications, second-order consequences, implementation problems, and their fit with existing policies.

Separated from society by virtue of their directive positions, officials prefer policies that help structure and heighten their autonomy. The institutionalized mitigation of societal constraints not only maximizes the translation of the officials' preferences into public policy, concomitantly satisfying their underlying interests and values, but it also allows officials to act autonomously with less risk, cost, and effort; attend to their internal affairs; experience a sense of secure tenure, and see themselves as authoritative "state" officials.

Due to their institutional positions public officials have a several-sided interest in allowing conceptions of the public interest to shape their preferences. Despite the analytical scalpels and sledgehammer attacks to which this concept has been subjected, it is "real" insofar as it has real consequences. Because of the officials' "high," strategically situated vantage points and of the value they place upon a judicious self-image and respected role definitions, they come to hold distinctive public interest preferences. Taking advantage of the prestigious institutions within which their offices are embedded, they have an interest in articulating public interest pronouncements so as to gain popular support and ward off private pressures (and a psychological incentive to believe these pronouncements). The long-term, broad-gauged policies that are seen to benefit either society as a whole or its parts may involve the interests of separate state units, the autonomy and legitimacy of the state, the maintenance of the political and economic order, the application of technocratic decisional criteria, responsiveness to the concerns of politically disadvantaged groups, and the acceptance of current sacrifices on behalf of future generations.

Although used interchangeably up to now, the preferences of individual officials are not the same as the state's. The latter represents an amalgamation of the former; the officials' resource-weighted preferences result in a state preference after being aggregated in a conflictual or conciliatory manner. Here institutions enjoy double-barreled explanatory power. The officials' resources (and even some of their skills) are institutionally derived, and their effective use and impact are mediated by institutional norms and decision rules.

How then does the state come to act autonomously? There are five strategies whereby the state translates its preferences into authoritative actions. Ideally, the state does this by combining autonomy-enhancing capacities and opportunities with predominant (not necessarily widespread) societal support. Such a (doubly) strong state—and comparably situated officials in other states—is able to realize most of the collective, organizational, and individual interests and values that shape its policy preferences. The autonomy-support combination is represented by the first two of the five strategies:

1. A state that currently enjoys societal support may capitalize upon its autonomy-enhancing capacities and opportunities to solidify it. Public officials act to crystalize the societal preferences that converge with its own, foster deferential and otherwise acquiescent beliefs and norms, and reinforce issue-specific and diffuse political indifference.
2. When the state's preferences diverge from society's, a strong state makes the latter convergent with the former. Public officials convince those with divergent preferences to alter them, persuade them that it is not in their interest to deploy their resources, offer them practically costless benefits to neutralize their opposition, and mobilize the support of currently indifferent societal actors.

An independent state acts—and some officials in other states act—autonomously despite a divergence in state-society preferences. It is unwilling or unable to enlist predominant societal support.

3. Public officials may free themselves from societal constraints by deterring opponents from exploiting their resources, mitigating the extent and effectiveness with which they are deployed, and offsetting and circumventing the resources' constraining effects.

An independent state may also rely exclusively upon its inherent powers.

4. It may simply accept the sanctions (e.g., protests, investment strikes, voting switches) that are prompted by its authoritative actions.
5. It may use its coercive powers to threaten and repress opponents so as to obviate any challenges and sanctions.

Most states are able to act autonomously with at least some frequency despite divergent state-society preferences. Even democratically elected officials have over a dozen autonomy-enhancing options both for transforming societal preferences and for freeing themselves from societal constraints. With few of these options depending upon exceptional skills or unusual circumstances, some are regularly available and relevant to the issue at hand (Nordlinger 1981, 109–117, 128–39). The state's inherent powers are present by definition, and even a small coercive apparatus is regularly sufficient to threaten and repress opponents (Nordlinger 1987, 378–79).

Given divergent preferences, what is it, then, about the state itself that explains the frequency with which it acts autonomously and the strategy by which it does so? Interests and attitudes have already played their part in shaping the officials' preferences. That leaves their beliefs and perceptions regarding the costs, risks, and opportunities of autonomous behavior. These are captured by two subjective variables that are institutionally determined.

Resilience refers to the officials' perceived capacities for counteracting manifest and potential societal opposition. It varies in accordance with the state's differentiation and cohesiveness. A differentiated state can exploit its diverse skills, expert status, and substantive expertise either to alter societal preferences or to mitigate opposition efforts and their consequences. A cohesive state is singularly advantaged in being able to articulate a coherent and persuasive message for changing societal preferences and—with *its* resources being marshaled and coordinated—for fragmenting, neutralizing, and countering the deployment of private resources.

Insulation refers to the belief that the state is minimally dependent upon societal support—support that ranges from endorsements and demonstrations, through votes and campaign contributions, to the calling and calling off of labor and financial strikes. The belief that autonomous action is affordable in the absence of societal support varies with the state's cohesiveness. Dependence upon private actors is lessened when its resources are deployed in a "unitary" fashion, as some (dependent) officials rely upon the resources of other officials and so avoid being fully exposed to societal leverage and as private actors are unable to play officials off against each other. A cohesive state is also better able to insulate itself by a resort to force. There is less of a debilitating divisiveness brought on by officials who oppose coercive measures that contradict their regime norms and downgrade and degrade their own offices.

The statist perspective can then move to another level of analysis, from heuristically prior "statics" to the "dynamics" of the emergence, alteration, and maintenance of the state's internal features. The "historical cure" is not, however, taken indiscriminately. The focus is on the features that were more or less autonomously instituted by the state and affected its capacities for autonomous action in the past and present. It is here that the polity's stresses, crises, and disjunctures take on special import. And because the state is the first and foremost entity to confront, and be constricted by, external actors militarily and economically and depends upon societal extractions to finance its foreign policies, the international environment is especially salient in explicating structural changes and policies that heighten the state's autonomy (Nordlinger 1987, 385-86).

Last, but by no means least, a distinctive statist perspective provides several paths by which to "return" to society. As an independent variable, there is the impact of the state's consistently salient policies and institutional weight, past and present. Its recurring directives and activities, internal processes, structural arrangements, and symbolic accoutrements help shape political beliefs and norms; levels and strategies of participation and opposition; the political crystalization and alignments of ethnic segments, classes, and interest groups; and the latter's organizational contours, inclusiveness, and specialization. As an institution, the state also helps determine who gets what, when, and how. As an intervening variable between societal demands and policy decisions, it patterns the relative effectiveness of private resources and how they are deployed.

A statist perspective need not—and this one certainly does not—suggest that the state regularly has a greater impact upon society than society upon the state. But without in any way minimizing the importance of societal actors and variables, the state can advantageously be accorded analytical priority. Over and above the state's impact upon society, the "mainstream" is almost exclusively concerned with the aspects of society that directly, indirectly, or tangentially effect the state—its structures, performance, and policies. And a statist framework can readily help identify those with the most frequent and decisive impact. In terms of the present perspective, which societal actors and variables best explain variations in the state's autonomy? Given the identification of certain subjective variables (malleability, resilience, and insulation) as the immediate explanation of its autonomy and their dependence upon certain institutional variables (boundedness, differentiation, and cohesiveness), to what extent, in what ways, and on whose part, are these societally determined? When accorded analytical priority, a statist perspective can serve as a beachhead from which to reconnoiter and then move into the societal interior.

At that point not just the putative "polemic," but the entire question of statism's distinctiveness will have been left behind. And deservedly so, as research and theorizing bring state and society together in meaningful, mutually illuminating ways that parallel their policy interactions and patterned relations. Indeed, judging by the writings of Callaghy (1984), Hall (1986), Haggard (1988), and Goldstein (1988), which focus upon institutionally patterned state-society relations, Almond's "generation of young scholars" is already here.

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Gabriel Almond has struck again, and political science will be better for it. The state has returned. But return of the state to political science was in the air long before the SSRC-supported "return-to-the-state movement." As usual for foundations, the SSRC acted like the amateur investor who bought in after the stock was already high.

In other words, the state was never really out. It was only overshadowed. Taking myself as a case in point, although behavioralism was still dominant in the early sixties, I felt no sense of isolation as I shifted my own vocabulary from policymaking to policy, from decisions and issues to "areas of governmental activity," and from categories of policy to "functions of the state." At Chicago in the late sixties we coined *post-behavioral era* to suit our own strengthening inclination to stress institutional phenomena. David Easton published it first, in his 1968 APSA presidential address, and gave it a somewhat different definition; but even so, it had an institutionalist spin to it. In the early 1970s the state was already central to what was becoming a "Cornell school" of political science (in Cornell's "Department of Government"). That university's orientation can be seen even in Almond's citations, reaching from Sabine to more recent members and products: (alphabetically) Aberbach, Krasner, Lowi, Nordlinger, and Skowronek. Another Cornell colleague, Sidney Tarrow, joined me as cochair of the APSA program during Charles E. Lindblom's presidency (1981) to try to hasten the trend by adopting as the program theme, "Restoring the State to Political Science." Still another, Peter Katzenstein, was on the SSRC Committee. Vivian Shue is a China statist.

However, as "restoring the state" became something of a movement, I began to ask myself, "With friends like these, who needs enemies?" Leave aside my annoyance with the fact that the new and more self-conscious statists rarely cite those of us who immediately preceded them. In fact, as Almond argues, they tend to disregard almost as much as do the behaviorists the rich statist or institutionalist tradition that is part of the mainstream political science literature. The political scientists of the thirties, forties, and fifties were strong in their appreciation of the state apparatus. An important part of their training was in traditional political theory; and most of them also studied public administration. For example, most of the "household names" of the fifties and sixties either did their dissertations in public administration or did some early, formative research in that area. Examples drawn directly from Almond's notes include Beer, Burns, Dahl, Herring, Key, Latham, and Truman.

If a poll were run for the number one political scientist of the past 50 years, most of the candidates would be from the above list, and my own pick would be V. O. Key. Nevertheless, he was typical of his generation: They were all strong on empiricism and *they avoided theorizing.* I never heard any of them say this, but the pattern is too consistent to be accidental. Political science meant realism, empiricism, loyalty to what was seen now. The highest art was description, good narrative. Quantification was important, but it simply meant description taken to a higher and more sophisticated level. Explanation was important too, but that was nothing more than describing two or more things simultaneously, each in relation to the others. Things were quantified wherever they could be; Key and his predecessors—like Gosnell—were past masters at it. Until well into the 1940s, quantification meant aggregate data drawn from county voting statistics, census data, and individual data drawn from legislative roll calls and the few available public opinion studies. Much more data and much better mechanisms for analysis are around today, but frankly, I think there has been little improvement since the 1940s in the uses of politically relevant data. When individual data from sample surveys became available, there was no hesitation or resistance in embracing them. Key and Truman were in many respects the first behaviorists in political science, as can readily be seen in their articles, their SSRC papers, and opportunities to proselytize. But neither they nor the others can be called reductionists, either to the individual or to the society. They were empiricists, in the business of describing political reality and making as much sense of it as possible. And for them political reality was institutional reality—legislatures, executives, parties, policies, elections, and organized groups. Almond is more than merely correct when he insists that these political scientists never forgot that it was a state in one form or another they were dealing with.

However, Almond misses an important point (maybe because he was too close to it), which is that this great generation of political scientists was an atheoretical generation. I am not alone wondering how much greater a work *Southern Politics* (1984) would have been if Key had let himself be inspired by Karl Marx or any other theorizing political scientist—for instance, his colleague Barrington Moore—and had pursued to the fullest the theoretical implications of one-party systems, the suppression of blacks, class voting, and state government control ("state" control) under these circumstances. But theory takes practice, one practices only to reach a desired goal, and theory was not a goal or vocation of this generation of empirical political scientists. In fact, Key's most self-consciously theoretical book, *Public Opinion and American Democracy* (1961), was also his least lasting. Even Lasswell, the most theoretically inclined of that generation—and probably the most erudite—did not sit down and write a theoretical work. His publications were empirical (e.g., *Psychopathology and Politics* [1960]), encyclopedic (*Power and Society* [1950]), or methodological (inventing research topics or methods for others to pursue, such as content analysis, political psychology, policy science, political recruitment). My work for Sayre and Kaufman in New York in the mid-fifties led me at that time to the conclusion that if the wisdom, the insights, the hunches, and the operating generalizations of this generation of empirical political scientists had been collected in a vast oral history project, we might now be on the way to political theory rich enough to pull us out of the shadow of the Great Books. But no member of that generation would step forward and claim to be either the creator or the adherent of theory properly so called. Theory was not a vocation; to speak of oneself, indeed to think of oneself, as a theorist or as someone doing theory would have been an unnecessary arrogance.

There were already some signs of a change in self-conception in the late 1950s. Reference was more frequently being made to a distinction between normative theory (the sort of thing actually done by the great thinkers and studies by political scientists who call themselves political theorists) and empirical theory (an oxymoron intended to locate a place some safe distance from Aristotle for twentieth-century mortals to try to make their research findings outlast the next issue of the *Review).*

Among the many probable causes—or provocations—of the new tendency toward theory, the three I find most interesting are the behavioral critique, the power structure critique, and the critique implicit in the experience with research outside the Western tradition. The emerging urge for the new vocation of theory was, in my opinion, the best thing that could have happened to political science. But since it was new and since it was to a large extent a reaction to outside provocations, the results were not going to be complete or pretty.

Behavioral Provocation

Psychology came out of World War II as the hot field. It had made a direct contribution to the war effort, whereas political science as a discipline had made none. It was a more relevant science for the postwar period because it offered a more promising approach to understanding fascism. And psychology had made the most outstanding breakthroughs in systematic studies of masses. The ultimate affront came when the National Science Foundation was established, including psychology at the very core of science while leaving political science on the outside looking in. Since many behavioral psychologists were already looking at voting because of its firmness as a behavioral datum, it was quite easy for political scientists to jump on the psychology bandwagon and to look upon individual units of opinion and of choice as superior both to aggregate data and to impressionistic data used by empirical political scientists. For many purposes it was superior and still is. But every solution is its own problem, and the problem of emulating a sister discipline is the likelihood of picking up its theoretical perspective as well as its data and methods. From the perspective of any definition of the political, behavioral psychology is reductionist.

My own opinion is that this—not pluralism as such—is the main source of reductionism in political science. The most ardent behavioralists were the most ardently committed to the individual as a unit of analysis and the most impatient with institutions and history. Almond observes quite accurately that Bentley, the prime reductionist, lay virtually unappreciated between 1908 and his rediscovery by David Truman in 1951. Almond is also correct in his observation that although Bentley was sometimes cited, his reductionism to the individual or to the group was not really adopted. However, the behavioralists—though unaware of Bentley as such—would have made him proud. Bentley was crying out for a unit equivalent to the atom. That was the only way to eliminate "soul stuff" like state and sovereignty. Behavioralists found it. As "empirical theory" came into fashion, the behavioralists were ludicrous in their efforts to build from individual data to the structure within which the behavior was taking place, without taking into account any information independent of their survey data.

The most important theoretical universe for the behavioralists was "the political system," especially as elaborated in Easton's book by that name (1960). But note the great distance between the individual units (more attitudinal than behavioral) and the system, the concreteness of the individual data and the total abstraction of the political system, and the substantive emptiness of the political system. This, to those of us who were emerging institutionalists or statists, was the most frustrating part. Nobody, including Easton himself, looked inside the "little black box" on which inputs, outputs, and feedbacks operated. Easton tried such accommodations as "withinputs"; and I was flattered by his interest in my arenas-of-power scheme as a possible means of filling in the contents of the little black box. But that was one of the problems of the political system. Its very abstractness and generality made it too accommodating. It was everything, therefore it was nothing. Note finally that when Easton and Eastonites turned empirical within the systems context, they literally stepped outside the political system altogether and studied political socialization. Now the interesting thing about political socialization is precisely that it takes place at a point outside, where people are not yet in but are ready to enter the political system. No wonder we would soon think of political socialization as something akin to what some critics said (with less justification) of the music of Debussy—it opened up a new street that turned out to be a blind alley.

Power Structure Provocation

First was a critique of empirical political science by the political sociologists. Community studies had always been prominent in sociology, and many a young political scientist was inspired by reading *Yankee City* and *Middletown.* But with Floyd Hunter's *Community Power Structure* (1953) something new was added. First, it was plainly more systematic than its predecessors. Better yet, its methods made community studies replicable and its findings testable. And testing they needed: the greatest contribution of Hunter's study was not its strength but its flaws, which provoked responses from a great number of political scientists. There is no need to replay them here, but they galvanized what eventually became a movement within political science, not yet a movement to embrace self-consciously something called theory but at least a movement to strive toward something in the long run that might be more important, to lift the discourse. I stress that: to lift the discourse. The reaction in political science was not to reject the individual or the group as units or to reject social forces or to reject any existing data or methods. The reaction was to lift the discourse toward (or back toward) the institutions and controls that make politics politics.

If I were required to identify one person as a link between the atheoretical empiricism and behavioralism on the one side and the vocation of theory on the other, I would choose Robert Dahl. Dahl had already been up to something that might eventually have been called theory in his collaboration with Charles E. Lindblom in *Politics, Economics, and Welfare* (1953). And Dahl's graduate seminar at Yale was entitled, The Policy Making Process. During the year I took the seminar, 1954/55, Dahl tried out some of the ideas that were to become central to *A Preface to Democratic Theory* (1956); but there was no self-conscious pluralist theory that I can recall, and the readings for the year were from a mainstream literature deeply preoccupied with the institutions of government. There was nothing socially reductionist about anything we did. But something else had happened a few months before that academic year had begun, namely, the publication of Floyd Hunter's book. In what turned out to be a quite direct response to Hunter, Dahl proposed that we do a group project rather than individual research projects for our year-long assignment and that the group project be a "power structure" study of the nearby community of Guilford, Connecticut. Guilford was more manageable than the larger city of New Haven, and recent events involving local education policies made Guilford a more interesting place to study. This was a first exposure of Dahl as well as the graduate students to the study of the distribution of power.

However, before these ideas would culminate in *Who Governs?* (1961), Dahl was to write the theoretical foundations for it in *A Preface to Democratic Theory* (1956). I really doubt Dahl would have chosen to do it in that sequence; but as so often happens, our agendas are set by others, in this case the invitation by the University of Chicago to deliver the Walgreen lectures in 1955. The *Preface* was, to my knowledge, the first self-conscious effort to piece a consistent and sustained "democratic theory" together from what is known now (but was not known then) as the pluralist literature. As Almond reports, Truman, Latham and others had made some sage reflections; and Sir Ernest Barker, Francis Coker, and two men unfortunately neglected by Almond—Robert MacIver and Merle Fainsod—had made important observations about the independence and lack of independence of individuals and groups from the state. Dahl had pulled it all together.

It is significant that C. Wright Mills' *Power Elite* appeared the same year (1956). Just as Dahl's book was not a reaction to Mills, Mills' book was a reaction neither to Dahl nor, interestingly, to Hunter. Hunter was only twice cited by Mills, and only once substantively, to confirm Mills' view that "power does not reside in . . . middle-level organizations; key decisions are not made by their membership" (1956, 37). However, the pure coincidence of the publication dates of those two important books only serves to confirm the point, which is that theory as a vocation was now in the air. What we called political theory suffered atrociously in comparison to Marx, Mill, or Madison. But the vocation of theory was emerging, out of the struggle between elitism and pluralism.

Non-Western Countries Provocation

Most of the best field research on every country in the world was done by U.S. political scientists—and in many countries outside Europe U.S. scholars were doing the only empirical research. Almond (1960) himself was a significant factor especially in his cooperation with James Coleman on an approach to theory clumsily called structural functionalism, which tried to build on the irreducible (not at all reductionist) idea that every identifiable institution in society had to meet some basic social need (or "functional prerequisite") or else it—and probably the society itself—would decline or collapse. Structural functionalism is a very powerful notion; it is a fulcrum that can help lever empirical work toward a theoretically interesting statement. Far from being reductionist, this approach to a more sustained discourse takes the view that the organized and repeated patterns described empirically become an index to the society itself and, ultimately, to all societies.

Both pluralism and structural functionalism came under attack in the late 1960s not for being reductionist but for being biased in favor of the status quo. This was an accurate observation, coming from another theory source, the critics of the new democratic Left. This might qualify as a fourth provocation except that (1) it was, in political science, a direct offshoot of the Mills-type critique and (2) by the time these critics arrived on the scene, political science was already committed to theory as a meaningful vocation. The leftist critique strengthened all would-be theorists by making them more conscious that all theory has political implications; all theory is a political act. Self-consciousness, like everything else in the vocation of theory, takes practice.

My own work in the late sixties was very much a part of the critical reaction (Lowi 1969), and I continue to embrace not only the criticisms I made but many of the criticisms made by people to the left of me. But criticism does not require rejection. Pluralism and structural functionalism were vitally important in the history of political science. For political science to have a vocation of theory at all, we had to get started, and we had to make our mistakes. The beauty of pluralism and structural functionalism was that they were both political and were not merely a mimicking of the other social sciences.

The most serious problem of these early political science theories has, however, not yet been fully confronted. Although Almond recognized it, he was being too defensive to give it its due. This is what Almond referred to as society-centeredness. Almond quotes Skocpol as claiming that the pluralist and functionalist approaches viewed government "primarily as an arena within which economic interest groups or normative social movements contended or allied with one another to shape the making of public policy decisions. . . . Government itself was not taken very seriously as an independent actor" and Krasner as confirming this criticism: "Pure interest group versions of pluralism virtually ignore public actors and institutions. . . . Government may thus be seen as an arena within which societal actors struggle to insure the success of their own particular preferences, . . . [leaving] no room for anything that could be designated as a state as actor with autonomous preferences capable of manipulating and even restructuring its own society."

Skocpol and Krasner seem to me to be absolutely correct in this. If I am disappointed it is only because they did not cite others who made the same criticism a decade and more before them. Take the following example from my work of 1969:

> Competition and its variant, bargaining, are types of conflict distinguishable by the existence of rules. . . . But rules and their applications imply the existence of a framework of controls and institutions separate from the competition itself. Whether we call this a public or not, *there is a political context that is not itself competition* within which political competition takes place. . . . [And] rules and their enforcement do not merely exist. They must be applied with regularity and some degree of consistency if pluralist competition is to exist at all. This is administration. Administration is necessary to construct and to change the system within which pluralism is to operate, yet pluralism presupposes the existence of that favorable structure. . . . [An] administrative component must be there. . . . [or] neither the reality nor the theory of pluralism has any meaning. (Lowi 1969, 37–39, emphasis original).

And although the statist critics are correct that there is something socially centered about the notion of arenas "within which societal actors struggle to insure the success of their own particular preferences" (in Krasner's words, quoted by Almond) arenas were not always treated merely as "a cash register that totals up and then averages the preferences and political power of societal actors." Again with Almond, people did appreciate the state and state institutions before the bring-in-the-state movement came along. The concept of arenas is a good concluding point because it ties back into a basic flaw in the new statist approach.

In the first place, the concept of arenas is very much a part of state-centered, institutionalist political science. It has encouraged many political scientists to study public policy, and it has influenced many already policy-oriented political scientists to look more seriously at politics within a state-control context. One of its particular strengths is that there is more than one arena, reflecting the two statist realities, that the state (1) is not neutral and (2) is more than one kind of force in the society. That points directly to my understanding of the most serious flaw in the statist approach. If this is a restatement of one of Almond's criticisms, it deserves repetition: the statists hit on a good idea—all the better because it is an old one. But they went too far with it. They reified the concept. They somehow came to the conclusion that "the state" is a unitary, solitary reality that can be brought into theory as some kind of a measurable force. When the statists speak of "state structures," for example, they are getting into empirical realities and are, in my opinion, quite off base. They speak of relative degrees of autonomy (Nordlinger) and relative degrees of strength or weakness (Skocpol). And they tend to use policy case studies as a way of measuring state autonomy and strength even as they treat the state as a macrostructure within which the struggle takes place. States can in fact be strong or weak, and states can be more or less autonomous. But there is no single dimension of strength, and there is no single dimension of autonomy. In fact there are several dimensions of each, and any given state could be placed high or low on any of the dimensions of strength or autonomy. This is precisely why embrace of "the state" as a unitary concept will inevitably be poorly defined. There are too many characteristics of states, and too many of these characteristics are really continua, with every state placing at a different point on each continuum.

Experience with the new statist movement, enriched by Almond's insightful review, has led me to some conclusions about the state and its role in political science. State—like power—is a term of art. It demands a particular focus. It is a residual category, applied to the explanation when all known measurable and controllable factors have been exhausted. It is not a phenomenon that can be studied directly. Although studying power as such came to naught, political science would be virtually helpless without it. Nevertheless, power alone, even as an organizing concept in political science, was insufficient to drive the discipline, mainly because it was too situational, too likely to lead the observer away from control and from history—in a word, away from institutions. State is a worthy addition, not as a new variable but as a change of focus in political theory. State is really not needed so much in everyday empirical political analysis. But it does become significant when empiricists seek to lift their discourse toward a line of argument that permits them to join with, or engage, others who do not care for their empirical situation but care a great deal about how people are governed. The older generation of political scientists appreciated the state but can be criticized for two kinds of neglect: neglect of theory and neglect of responsibility. Theory means subordinating empirical experience to the long line of argument. Responsibility means subordinating experience *and* theory to a good constitution. I am not yet satisfied that the new statist movement has moved political science much further toward meeting either of those two needs. The realism of the older generation left them open to the criticism that they were ahistorical, nontheoretical, nonnormative, and, as a consequence, apologetic. The new statists may be bringing back the state, but for the wrong reasons. The point is not to make the state a variable but to make political science, through a new and higher level of discourse, a discipline worthy of constitutional democracy—scientific, theoretic, historical, and critical.

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Almond's essay, "The Return to the State," provides us with a useful opportunity to take stock of the debate on the state that has by now been in progress among scholars of politics for a decade.

With his habitual elegance and with the authority that derives from his acknowledged mastery of the vast field of postwar scientific thought in the political field, Almond examines the logic that has and does provide the basis for the statist critique of the mainstream of political science; that is to say, he does not limit himself to a confutation of one or more of the constitutive arguments of such a critique but rather directs his attack at its very foundations, its conceptual structure and analytic method. Almond has set himself an important and difficult task. If he achieves a degree of success, the terms of the theoretical debate between "mainstream" and "statist" political scientists will have to be substantially revised.

Almond's attack has three objectives: to demonstrate (1) that an interpretation in society-centered terms of pluralist and structural functionalist postwar literature, an interpretation that has undoubtedly served as a "rallying point" for the various studies that have come to constitute statist literature, is baseless; (2) that the claim by statist literature that it has formulated a new theoretical paradigm better suited than those it criticizes to interpretation of political processes is baseless—which is further demonstrated by the fact that in practice such a "new" paradigm is a retrograde step for analysis, insomuch as it displays the limits of reductionism and determinism that it erroneously attributes to the mainstream of political science; and (3) that the benefits that may (and do) accrue to political research as a result of this "return to the state" have been far outweighed by its cost to the scientific community. Whereas Lindblom (1982), in his discussion of radical political theory, reaches the conclusion that mainstream political science can be enriched by the critique that the theory had directed against it, Almond, although motivated by the same spirit, reaches a different conclusion: Arguments that have for some time now belonged to mainstream political science are advanced by the statist critique couched in much more confused and contradictory terms. In short, *Il faut tourner la page.*

Has Almond Achieved his objectives? As far as the first is concerned, it seems reasonable to conclude that he has been unequivocally successful. With patience and with a meticulous use of quotations (thus employing a debating style that deserves praise), Almond demonstrates that the statist critique can be partially justified only when it applies to classical pluralism, which certainly underestimated the amount of autonomy available to the state in the policy-making process. In Almond's view, however, this underestimation has by no means been a feature of postwar literature—when, of course, by *state* is meant the government, or, better, that group of public officials and elective personnel operating in agencies endowed with decision-making resources (Easton 1960, chap. 4). The (particularly effective) argument that Almond adopts in reaching this conclusion consists of the consideration and discussion of mainstream literature as a whole (or at least its constitutive components—political theory, comparative government, American studies, and European studies). Despite claims to the contrary, Almond argues, this literature does not conceive of the state (the government) as a "cash register"; it has in a more concrete fashion assigned to empirical investigation the task of identifying the nature of the specific relations among (both private and public) actors in the various contexts of policy examined (e.g., most notably, Lowi 1964, which is extensively and positively quoted by several authors of a statist orientation).

The mainstream of political science has therefore worked with a paradigm that treats the relationship between state and society in terms of an interactive system of reciprocal influences. On the basis of this paradigm categories of analysis have been derived (among which *political process, policy area, and political structure* have been of especial importance) that have enabled researchers to reconstruct the precise relations of influence between the two entities, although they have recognized that the relationship is an asymmetrical one ("The policeman can stop traffic; drivers cannot stop the policeman"). Unlike previous legalist and formalist tradition, which conceived of the state as an abstract set of norms and institutions, postwar literature, with its roots in the behavioral revolution, has "denuded the term of most of its implication" (Easton 1968, 283). The state, "reduced simply to a neutral and empty conceptual shell for identifying the actors in the international sphere" (p. 283), has therefore been turned into a methodologically operative concept, an individual unit of analysis that can be subjected to empirical investigation. While, "as a result of this revolution in methods and data, political science," for Easton, "has begun to break away in earnest from an institutional orientation" (p. 291) and "processes, rather than structures and institutions, have begun to appear as the major guidelines for political research" (p. 291), Almond argues that the mainstream of postwar political science has continued to address those key questions that have traditionally inspired political thought (including questions relative to political authority and the exercise of rule), and in fact it has provided more sophisticated and more satisfying answers to them. Easton wrote 20 years ago, "The new conceptualization of the key problems with which political science deals does not in itself preclude the continuation of past concerns in political research" (1968, 288).

Almond's reply undoubtedly rescues pluralist-structuralist functionalist literature from the criticism of determinism that had been leveled at it and obliges the proponents of such criticism either to concede the point or to reformulate it with much better documentation and with references to that general body of scientific knowledge that the literature presently incorporates. However, it should be added that Almond's treatment of Marxism can hardly be regarded as exhaustive: an author from that field of study needs to make a direct contribution to the debate. In fact, the complexity of Marxist debate over "the capitalist nature of the state" and "the relative autonomy of the state" precludes an excessively simplified reading (cf. Alford and Friedland 1985, pt. 3; Carnoy 1984; Kasinitz 1983; and Zeitlin 1980), not only because different positions have emerged during the debate (e.g., Block 1977a, 1977b, 1981 and Offe 1975, 1984), but also because, as Easton points out, some "Marxists have effectively given up the concept [of the state] insofar as they use it simply as a rough substitute for governmental apparatus, or what I would prefer to label the political authorities" (1981, 320). If this is true, then Marxist analysis, too (at least as in some respects), should now be ready to employ the instruments and methods of an empirical theory, which by its nature is hard to reconcile with a determinist framework. However, I would argue that Almond's assessment of the society-centered nature of Marxism could also be subjected to more exhaustive debate.

Can we say that Almond has achieved his other two objectives with equal success?

State and Political Institutions

Almond's reply has lent implausibility to any claim that the mainstream of political science does not possess a concept of the state (of the government) as an actor. Much more plausible, however, appears the criticism that mainstream political science has undervalued the importance of the institutional components of the political system. Easton (1968, 292) has stated that among the political scientists whose literary output since the war now constitutes mainstream political thought the conviction has grown "that the specific institutional setting is of less importance than are the generic forms of behavior that appear in the various institutions, at least for better understanding the way in which the institutions themselves function." Thus the study of executive and legislative, of judicial and administrative, institutions becomes chiefly the study of their specific forms of behavior. And these, in turn, reflect the dynamics of the behavioral interaction among individuals and subunits as they perform the roles, also specific, that constitute such institutions (cf., esp., Eulau 1968). The political structure, by necessity, therefore comes to be regarded principally as a relation among roles (rarely as a relation among institutions and never as a relation among organizational principles). This, it seems to me, is essentially Almond and Powell's (1978, chap. 3) position, although they provide a much more refined treatment.

Almond is probably correct to criticize the vagueness with which certain concepts ("state" and "society," "weak state" and "strong state") have been introduced into the scientific debate by some scholars of a statist orientation. However, he does not appear willing to draw any analytical conclusions as to the reasons for the revival of concepts that seemed to have been definitively superseded in the scientific development of political science.

In particular, two phenomena—the enormous expansion of public intervention in the postwar period among advanced democracies in general (with their well-known national differences) and the relative coherence with which this intervention was conducted (to the point of giving shape, for example, to specific national models of welfare state)—seem to have altered the nature of the relations between public and private. (An enormous amount of thinking on the subject has given rise to a genuine literature of renewed political economy centering on analysis of the "state" and the "market." See, e.g., Flora and Heidenheimer 1981; Goldthorpe 1984; Lane 1985.) Moreover, the development of the welfare state was accompanied by an increased complexity of the (chiefly state) institutional arrangements (Rose 1984), which made manifest the degree of control—in terms of conditioning and not necessarily of determination; the distinction is methodologically important and empirically verifiable and greatly facilitates interpretation of the various components of statist literature—exercised by these arrangements over both social dynamics and the political processes themselves. It seems reasonable to suppose that the reemergence of the problem of the state has been due not to the efforts of scholars of a statist orientation but to these real processes and the questions they have raised.

Elkin has written (1985, 2), "But several features of twentieth-century democratic political economies have made this [mainstream] view of the connection between society and the state increasingly problematic." He therefore asks (p. 4), "Has not the rise of America as a world power in the atomic age prepared us to think in statist terms?" Perhaps it is no coincidence that the reintroduction of the state into debate in political science has been due to scholars of political economy (leaving aside authors on international politics, who have in fact never relinquished the term). For example, Dunleavy (1987, 470) has pointed out, "The ageing of the conflicts between institutional and behavioral methods, and between the apostles of a 'positive' political science and defenders of normative political thought, has coincided with the strong revival of two political economy approaches closely linked with different theories of the state. The first is public choice theory. . . . The second political economy approach is neo-Marxist." Elkin (1985, 6) notes that dissatisfaction with the latter approach has led to "an eclectic political economy whose starting point is typically the state officials themselves or whatever institutional arrangements are labeled the state."

The clearly central role that state institutions have come to play within the various national political systems seems also to have encouraged a revival of interest among scholars in the historical role played by these institutions in the shaping of the identity of political actors. (See Katznelson 1981; Katznelson and Zolberg 1984; Shefter 1978; Salisbury 1979 and Wilson 1982 address the labor unions' organizational characteristics, which, together with those of state institutions, have made a neocorporatist policy impracticable.) This awareness of the increasingly institutional nature of contemporary politics has not translated, and does not necessarily translate, into a state-centered paradigm, even if this awareness has in many cases attributed a causal position to political institutions—though part of the literature did move toward a (deterministic) state-centered paradigm. March and Olsen (1984, 738) have recently summarized the new analytical approach as follows:

> "Without denying the importance of both the social context of politics and the motives of individual actors, the new institutionalism insists on a more autonomous role for political institutions. . . . Political democracy depends not only on economic and social condition but also on the design of political institutions. The bureaucratic agency, the legislative committee, and the appellate court are arenas for contending social forces, but they are also collections of standard operating procedure and structure that *define* and defend interest. They are political actors in their own right." (my emphasis)

One gains the impression that Almond not only underestimates this reawakening of interest in political institutions but also the effort that has been made—which is certainly to be found in a part of statist literature—to specify the reasons for the relative coherence displayed by specifically state political institutions in the course of their evolution.

The point has been made in the following manner by Benjamin and Duvall (1985, 55), who have a somewhat restrictive view of the state as a set of administrative agencies, or at least of what they call "State I": "It makes little sense to conceive of the state simply as the set of disparate administrative agencies, each doing its own separate thing. The state is that set of agencies as a coherent whole, or totality. Rather than being concerned with what the separate agencies do, the theorist of the state must be attentive to the organizing principles that give coherence to the disparate set. It is a matter of seeing ordering principles in apparent chaos." Such an analytic approach sees political institutions not only as actors and structures that define interests but also (in the case of state institutions) as being generated by principles of organization that allow their identification as parts of a relatively coherent whole, or better, as components of a specific subsystem.

These are certainly not extraordinarily innovative conclusions for social science. For example, 20 years ago Eisenstadt (1968, 410) stated that "institutions . . . can be defined . . . as regulative principles which organize most of the activities in a society into definite organizational patterns" and took pains to point out that "institutions are very close to, but not identical with, groups or roles that are organized around special societal goals or functions. Thus, not only are the principles of political regulation effective with regard to those groups whose major function is some kind of political activity—be it administration or mobilization of power—but they also regulate various aspects of groups whose predominant goal or function is economic, cultural or educational." Therefore the revival of interest in political institutions seems to derive from a need to specify what we might call the "institutional structure" of the political system, an important component of which is undoubtedly the structure of the state institutions. Moreover, a preference for an institutional (specifically, state) approach to the interpretation of political processes is not alien to the mainstream literature of political science: consider, for instance, Rokkan (1970) and Tilly (1975) and the role played by institutional factors in their interpretations of political development and, especially, Huntington (1968) and his fundamental analysis of the processes of institutionalization.

It would be interesting to examine the emphasis—to be found in the best statist literature—on the institutional structures that organize the political process, policymaking in particular. However, I shall mention only the research conducted by Katzenstein (1978) into international economic policies (published more than 10 years ago), which is still one of the most important accounts in the literature. In presenting his research, Katzenstein justifies his use of the statist approach by citing the arguments advanced by the Weberian sociologist Bendix and his colleagues (1973, 11), according to whom "social life is structured—not exclusively of course, but structured nonetheless—by just those formal institutional mechanisms. To disregard such structures at least implies the belief that social reality is essentially amorphous. This does not mean that institutions work as they are intended to work; *it does mean that they have an effect*" (my emphasis). The problem that now arises, I believe, concerns the specification of the characteristics and limits of such a structure, especially of its state component, which—for objective reasons—is the most important. Blondel (1987, 483-84) writes on the importance of state institutions: "It may be true that political activities within the state are more important than other political activities—it may also be true that the state may have the power to regulate the political activities which take place in other organizations: for instance, the state may specify that only certain types of associations are allowed or that associations may engage in certain types of activities. This may result in political activity within the state being dominant; it does not result in the end of political activity in other bodies." We might argue over the use of the term *bodies,* but the problem of the relationships among subsystems seems to me to have been well put in conceptual terms.

In dealing with the problem of specifying the characteristics of the state, authors of a statist orientation have put forward analytical solutions that appear neither empirically convincing nor theoretically coherent. Nevertheless, they are solutions that might be developed in an interesting way. Generally speaking, many of these authors use, in combination, two approaches to the state: the first treating it as a set of administrative agencies, the second as an institutional and legal order structured on precise, and historically determined, organizing principles. These latter are visible principles in the sense that they institutionalize themselves in norms, regulations, procedures, resources, and relations; and public action must therefore be conducted within the *constraints* and *opportunities* defined by the characteristics of that process of institutionalization. In institutionalizing themselves, these principles tend to become relatively permanent through time, even if environmental or political changes within the system may lead to their—albeit slow and partial—transformation. Understood in this sense, state institutions perform not only regulative but also selective functions; not only do they have functions that define interests but also functions that distinguish between interests that are legitimate and those that are not. To argue, as Benjamin and Duvall do (1985, 36), that "those organizing principles, in effect, constitute the structure of the state" does not therefore entail a return to an abstract theory of the state—provided that an accurate specification is made of the institutional structures embodying the principles. It is worth reminding ourselves that the usefulness of the approach has been demonstrated by a number of comparative studies of both the development of welfare states (Esping-Andersen and Korpi 1986) and of developing countries (Duvall 1983).

I would maintain, therefore, that Almond has plausible reasons for concluding that statist literature has not formulated a paradigm that is sufficiently coherent or sufficiently alternative to that of mainstream political science (although March and Olsen [1984, 747] have pointed out that "The institutionalism we have considered is neither a theory nor a coherent critique of one. It is simply an argument that the organization of political life makes a difference"). More controversial, however, in the light of the preceding brief discussion, is his criticism, via Krasner, of the whole of statist literature for having taken up a "thoroughgoing determinist position."

State and Political Science

We can draw two basic conclusions in order to discuss the third of the objectives that Almond set himself. The first, statist literature also comprises a variety of points of view, the methodological and conceptual differences among which need examining. Together with approaches that probably presume they are able to transform the state into the analytical premise for a new interpretative model of politics, we find other approaches that, more soberly, seek to encourage a reconsideration of the importance of political institutions (and of state institutions in particular) within political systems—past as well as contemporary. March and Olsen (1984, 747) write, "It would probably be more accurate to describe recent [statist] thinking as blending elements of an old institutionalism into the noninstitutionalist view of recent theories of politics." Second, if, in examination of statist literature, we use a criterion that is able to highlight the distinction between contributions that see the state as an (indeterminate) "concept" and those that see it as a (determinate) "institution," it is possible to lay a different basis for comparison between mainstream and statist literature.

Certainly, comparison on these terms would not be a linear or painless undertaking. After all, the mainstream of political science has its theoretical and methodological roots in a long process of scientific reassessment, one consequence of which has been that the analytic framework has rejected the state understood as a concrete institution. Twenty years ago Fried (1968, 144) summarized the problem as follows: "The revolution in thought, which, at least temporarily, undermined the analysis of the state as a *complex of concrete institutions,* began towards the end of the nineteenth century with the emergence of functionalism. It was in the twentieth century that new ways of looking at the state, stimulated by the parallel development of behavioral and operational approaches, began to show competitive degrees of refinement" (my emphasis). The state was replaced by the political system; and interactions and processes internal, and above all external, to formal structures took over from the old predilection for laws, rules, commands, and order. Thus the student of politics was provided with tools and methods that then invested his or her inquiries with scientific integrity. The study of politics thus attained the (recognized) status of a specific social science. It is in nobody's interest to relinquish these achievements.

Nevertheless, it seems legitimate to ask whether political research has not, in the course of its scientific evolution, denied itself fruitful areas of investigation by abstaining from analysis of the state as a complex of concrete institutions. In other words, does a "return to the state" embody a return to the study of the legalist forms of political power—a return to the elaboration of abstract concepts—or does it represent a greater sensitivity among students of politics towards institutional arrangements and their importance for, if not centrality in, the evolution of political systems? Pasquino (1968, 121) summarizes the problem thus: "Contemporary political science, even in its best formulations, has not given enough attention to structural-institutional problems and *their impact on the dynamics of the political system*" (my emphasis). Such attention, however, characterizes the two volumes (Castles and Widenmann 1986 and Katz 1987) on "The Future of Party Government" sponsored by the European Institute of Florence. Almond himself (1987, 246) has recently written in a discussion of functionalism that "from the methodological side it is viewed by some as reductionist, as obscuring the importance of the state and *political institutions*"—as opposed, that is, to the politicoadministrative personnel who operate within them.

There do not seem to be methodological obstacles to a translation of the definition of the state as a "legal-institutional order" into a definition that sees it as a specific subsystem, one that is part (but a particularly important part) of the more complex political system. Many features distinguish this subsystem from others.

Of course, this specification is necessary only if one recognizes that such a subsystem enjoys a special stature with respect to the other subsystems within the political system; a stature that it derives, inter alia, from both the specific features of its internal political structure and from the specific tasks performed by its institutional structure in relation to the overall political system. In its turn, this raises the problem of the specification and the definition of such concepts as "political structure" and "institutional structure" and the relation between them—a problem that does not lend itself to any easy solution. Nevertheless, the mere act of pointing out these distinctions enables us to reconstruct the reasons why the state subsystem *de facto* performs tasks that the other subsystems do not. In this way (and in a nondeterminist manner) it is possible to reintroduce into the analytical framework those functions of conditioning and legitimation that the institutions of this subsystem(in its most general activity) perform vis-à-vis the processes and actors of the political system.

Some recent remarks by Easton may help to make the point clearer: "[The political system is] those social interactions and *institutions* through which a society makes decisions considered binding by most members of society most of the time" (1987, 478; my emphasis). Now this approach to the political system does not entail an analytical use of institutions in the exclusively behavioral sense. In other words, this approach does not appear to require inhibitory mechanisms for the formulation of a dynamic model able to establish the relations (and empirically verify them) between (individual) choices and (institutional) constraints. If certain inflexibilities in the formulation are relaxed, there seems to be no logical irreconcilability between approaches that see the political process chiefly as interaction among individual actors who make choices and those approaches that relate the choices made by actors to the fulfillment of duties and obligations. In reality, actors behave in structured contexts (contexts that do not determine the outcome of an action but influence it), the characteristics of which can be traced back principally to the nature of the political institutions that are, precisely, "most closely related to the making and implementing of political decisions." It seems difficult to deny that in the history of modern societies state institutions have enjoyed—and enjoy even more in today's welfare society—a particularly privileged position in the "making and implementing of political decisions."

It is probable that—more than just a debate between contrasting methodological positions—the controversy over the state will continue to take the much weightier form of an irreconcilable divergence between two opposed theories of democracy—one empirical (Anglo-American) and the other rationalist (Continental European)—a divergence that has recently been summarized by Sartori (1987, 53): "Rational democracies are constructed deductively . . . whereas the construction of empirical democracies largely results from . . . inductive elements." For rationalist theory the state is the "fixed structure," the "depersonalized, impersonal, juridical form" that can give organic shape to the evolution of the democratic regime. For empirical theory the state is an entity behind which there stand "concrete persons" who "govern" the process of trial and error that makes the constant improvement of the democratic regime possible. However, it seems reasonable to claim that today the debate on the state manifests itself in consistently new terms. If this new development is not accepted, then statist and mainstream political scientists will continue to speak different languages, with no shared vocabulary with which to communicate; and also, as Dunleavy (1987, 471) has written, "the recent emphasis upon the theory of the state does not look as if it will disappear quickly," then the outcome threatens to be an ever-increasing rigidity in the various positions within the discipline.

Almond's essay "The Return to the State" might encourage reflection on the subject that would help to avoid this situation. It clears the ground of controversies that until very recently appeared important and helps to delimit the extent of the divergences among the various positions. If it is followed by further contributions able to shed light on both the present deployment of positions in statist literature and the significance for the discipline as a whole of a revival in institutional analysis (and of course in administrative and institutional history as well) —contributions not couched in exclusively individual terms—then the basis could be laid for a new and interesting phase of theoretical debate—a debate that could lead to the overcoming of several important limitations present in both approaches. I submit that Almond has only partly achieved his objectives (successfully the first, less satisfactorily the other two). Nevertheless, his contribution, if taken up, will certainly come to be regarded in the future as an important step forward in the debate between mainstream and statist political scientists.

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RESEARCH NOTES

MEASURING POLICY CHANGE IN THE U.S. SUPREME COURT

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Measuring the U.S. Supreme Court's policy changes is complicated by change in the content of the cases that come before the Court. I adapt from earlier scholarship a method to correct for changes in case content and use this method to measure change in the Court's support for civil liberties in the 1946–85 terms. Analysis based on this method indicates that because of changes in case content, the average difficulty of reaching a pro-civil liberties result varied during that period. With corrections for case difficulty, the Warren Court of the 1950s appears to have been more conservative, and the Burger Court more liberal, than patterns of case outcomes themselves suggest. This method, while imperfect, has utility for the measurement of policy change in the Supreme Court and other institutions and thus can serve as a building block in analyses of the processes and determinants of change.

Over the past two decades, as Republican presidents have appointed a series of Supreme Court justices, interest in the Court has focused on policy change. Particular attention has been paid to the Court's growing conservatism in civil liberties. But scholars and other commentators disagree on the extent of the Court's movement to the right.

This disagreement results in part from the difficulty of measuring policy change in institutions such as the Supreme Court. The content of the cases that the Court decides is constantly changing. Thus it is not easy to compare the ideological positions that underlie the Court's decisions in two different periods.

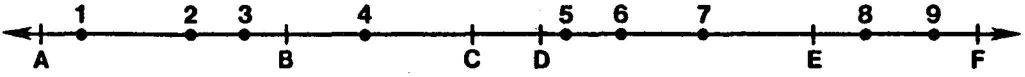
There may be no entirely satisfactory means to overcome this difficulty in a policy area as broad as civil liberties. But even imperfect methods for measurement of policy change can facilitate analysis of the processes and determinants of change. I present one method to measure policy change and use that method to examine changes in the Supreme Court's support for civil liberties in the 1946–1985 terms.

Conceptualizing Policy Change

At the outset, the concept of policy change should be clarified. We might measure change simply in terms of changes in the pattern of outcomes. If the Supreme Court ruled in favor of civil liberties claims 60% of the time in one period and 40% of the time in a later period, we could say that the Court's policies had become more conservative by that 20% difference. But such a calculation fails to take into account changes in the content of civil liberties cases.

The difference between change in outcomes and policy change can be described in terms of positions on an issue dimension, depicted in Figure 1. Assume that cases in an issue area fall along a single continuum for all justices. Each justice has an *i*-point representing the justice's ideal policy, and each potential case outcome has a *j*-point. In each case, the vote of each justice is determined by the relationship between that justice's *i*-point and the *j*-points representing alternative outcomes. If only two alternative outcomes

Figure 1. Representation of Ideal (i) Points for Justices and Midpoints between j Points for Cases on a Single Dimension



Note. Numbers represent *i*-points on the dimension for each justice, letters represent the midpoints between the (unshown) *j*-points for the two alternative outcomes in each case. If this is a liberal-conservative dimension (e.g., high support for civil liberties vs. low support for civil liberties), each justice will cast a liberal vote in each case in which the midpoint between *j*-points is to the right of the justice's *i*-point. Thus, the vote in case *A* would be 0–9 against a civil liberties claimant, while the vote in case *F* would be 9–0 in favor of a claimant.

exist (e.g., affirming or reversing a criminal conviction), a justice will vote for the more liberal alternative if the justice's *i*-point is to the left—the liberal side—of the midpoint between the two alternative policies. The Court's decision will be liberal if a majority of justices have *i*-points to the left of that midpoint.

From this perspective, changes over time in patterns of outcomes could occur in three ways (see Asher and Weisberg 1978; Baum and Weisberg 1980; Glenn 1977).

1. *Personnel change.* The pattern of outcomes changes because some justices are replaced by others who have different *i*-points on the dimension.
2. *Member change.* The pattern of outcomes changes because the *i*-points for some continuing justices shift.
3. *Issue change.* The pattern of outcomes changes because a new set of cases has a different mix of *j*-points from the earlier set. If the *j*-points generally move to the left, it becomes more "difficult" to cast a liberal vote and the proportion of liberal outcomes declines.

Changes in outcomes that result from issue change should not be interpreted as policy change, because the Court would still reach the same outcomes if it were confronted with the same cases. But outcome changes that result from personnel and member change qualify as policy change, because the Court has shifted rather than the cases. The analytic task is to separate out issue change from these other two sources of change in case outcomes, in order to ascertain the extent of actual policy change.

A Method for Estimating Issue and Policy Change

Within the body of work on legislative and judicial policy change, some scholars have taken into account the effects of issue change. One approach, used primarily in studies of the Supreme Court, is to control for variables related to issue content (e.g., Atkins and Sloope 1986; Ulmer and Thomson 1981). The most comprehensive of these efforts is Segal's study of search-and-seizure cases (1984, 1985), which was highly successful in separating the impact of a variety of case facts that influenced outcomes from the impact of personnel change and member change. For areas as broad as civil liberties, however, it is far more difficult to measure and control for case content in this way. For such areas, another approach is needed to estimate issue change.

An alternative approach is to infer the content of issues from votes themselves (Burstein and Freudenburg 1978). Two studies in quite different areas developed similar methods of this type. One was a study of congressional voting change on economic and welfare policy by Clausen and Cheney (1970, 148–51). The other was an effort to compare the skills of batters in baseball who played during different eras (Cramer 1980). The general method used in these two studies is applicable to institutional policy change in broad issue areas such as civil liberties, and the method used here is adapted from those studies.

Three assumptions underlie the estimation method to be used:

1. Every civil liberties case falls along the same single policy dimension for every justice, and every justice places the alternatives in each case at the same *j*-points on the dimension.
2. Each justice's ideal point on the civil liberties dimension remains constant throughout the justice's career.
3. Justices' votes are based solely on the relationship between their ideal, or *i*-, points and the *j*-points for alternatives in the case.

Although these assumptions are common in research on judicial decision making, their imperfections are obvious. A method can be based on these assumptions, however, because their deviations from the reality of Supreme Court behavior appear to be limited and generally unsystematic. For instance, all civil liberties cases do not fall perfectly along the same attitudinal dimension; but dimensions within civil liberties correlate highly, and a single dimension describes the voting pattern in these cases fairly well (Schubert 1963, 1974). Justices undoubtedly differ in their placement of some cases on a policy dimension, but the high scalability of votes in specific issue areas suggests that these differences are limited (Rohde and Spaeth 1976).[1] Similarly, it appears that some justices' policy views shift over time ("Changing Social Vision" 1983), but past studies (Schubert 1974, 159) and the data used for this study suggest a high degree of stability for most justices.[2] Thus, a method resting on these assumptions is appropriate if imperfect.

The assumptions rule out the possibility of member change, leaving only personnel and issue change. Thus, changes in case outcomes can result only from issue change and personnel change; changes in individual voting can result only from issue change. Because the issue change component does not represent true policy change, the personnel change component represents the full extent of policy change. Since changes in individual voting result only from issue change, we can obtain a measure of true policy change by subtracting changes in individual voting (issue change) from changes in case outcomes (personnel change plus issue change).

To use this approach, several steps were taken. First, cases from the 1946–85 terms with civil liberties issues were identified and the votes of each participating justice coded.[3] Cases were eligible for inclusion if they contained a civil liberties issue and at least one page of opinions was issued. To approximate the unidimensionality more closely, cases were excluded if civil liberties claims were in conflict or (with a few exceptions) if civil liberties claims were made by businesses.[4]

Both unanimous and nonunanimous decisions were included. This decision was appropriate to the purposes of the study, because both kinds of cases define the positions of the Court and of individual justices. An analysis from which unanimous decisions were excluded produced results similar to those of the main analysis that includes those decisions.

Justices' votes were coded as pro– or

Table 1. An Example of the Procedure for Calculation of Corrections

Natural Court	Support Scores								
	Black	Douglas	Clark	Warren	Harlan	Brennan	Stewart	White	Court
9	81.7	93.4	51.7	85.1	40.4	83.1	59.7	61.7	79.1
10	64.0	90.9	49.1	76.8	36.1	74.3	47.8	55.6	65.2
Change	-17.7	-2.5	-2.6	-8.3	-4.3	-8.8	-11.9	-6.1	-13.9

Note: Median change for continuing justices = -7.2; correction = +7.2; policy change for Court = -13.9 -(-7.2) = -6.7.

anti-civil liberties or as mixed. The coding was based on the potential outcome of the case for the party making a civil liberties claim—whether that party would win the case if that justice's vote prevailed. A justice's vote was coded in relation to the range of possible outcomes for the parties, not in relation to the positions of other justices, because our concern is absolute levels of support for civil liberties.

The votes of each justice and the decisions of the Court as a whole were summed for each "natural court" to produce a civil liberties support score, the proportion of individual votes or of decisions favorable to litigants with civil liberties claims. A natural court is a period that begins with one personnel change and ends with another. In three instances, two or three natural courts were combined to create a period in which the Court decided at least 75 civil liberties cases.[5]

To measure issue change, support scores for justices were compared across natural courts. For each adjacent pair of natural courts, a continuing justice's support score for the first natural court was subtracted from the justice's support score for the second court to produce an individual difference score. The median of the difference scores for the seven or eight continuing justices then was calculated. In line with the logic described earlier, that median then was subtracted from the change in the Court's proportion of pro-civil liberties outcomes to produce a measure of policy change between the two natural courts.

This procedure is illustrated in Table 1. In the example shown, the median of the justices' difference scores between the ninth and tenth natural courts was −7.2%.[6] Based on the assumptions, this figure indicates that it had become slightly more difficult to support civil liberties because the average *j*-point for cases had moved to the left. This −7.2% figure then can be subtracted from the change in the proportion of pro-civil liberties outcomes between the two periods: −13.9 − (−7.2) = −6.7. This result indicates that the actual policy change was only about one-half as extensive—though in the same direction—as the change in outcomes. The −7.2% figure, with the negative sign made positive, also can be used as a correction for case difficulty in the Court's support score; and the corrections can be cumulated across periods to allow comparisons of the Court's "true" support for civil liberties among all the natural courts in the 1946–85 terms.

Findings

We can begin consideration of the findings by examining the actual pattern of outcomes in civil liberties cases during the 1946–85 terms. The data are shown in Table 2.

The pattern of outcomes generally coincides with what we would expect. With the departures of the most liberal members of the Vinson Court in 1949, the Court's support for civil liberties dropped sharply. The early Warren Court of the 1950s supported civil liberties claimants more often, and the support level increased again with the 1962 appointments that created the late Warren Court. The successive Republican appointments beginning in 1969 were followed by successive declines in support.

There are, however, some anomalies. In the late Warren Court, a major drop in civil liberties support followed the replacement of Arthur Goldberg by the like-minded Abe Fortas. The Court's collective policy views were probably most favorable to civil liberties after Thurgood Marshall's appointment in 1967, yet this was not the period of the greatest civil liberties support.

The Burger Court's record might be considered the greatest anomaly. The 38% support level from 1975 on ranks well below the level in the early Warren Court and at about the level of the late Vinson Court. Yet even in its last decade, the Burger Court's doctrinal positions were often quite favorable to civil liberties. To equate that Court with the late Vinson Court, which appeared to be much less friendly to civil liberties doctrinally, seems inaccurate.

Table 3 shows the changes in support levels that result from the correction procedure. The corrections for each pair of natural courts, shown in the "correction from previous" column and cumulated in the "cumulative correction" column, are of interest in themselves as measures of issue change. The figures indicate that case difficulty declined considerably in the late Vinson Court and a bit more through the 1957 term. It then increased in several increments, until the cases decided in the 1969-70 terms were nearly 20% more difficult, by the metric used here, than those decided in the early Warren Court. Case difficulty declined in the early 1970s, but most of that decline was canceled out by later increases.

The most important pattern is the considerable growth in case difficulty between the late 1950s and late 1960s. This growth might be expected. As it moved into several civil liberties fields, the Warren Court tended to deal with the easier cases—those with relatively clear civil liberties violations—first. Gradually, the Court took on the more arguable cases, the ones on which even liberal justices might disagree. As a result, in cases decided by the Burger Court, pro-civil liberties decisions were more difficult to reach than in the early Warren Court. The general consistency of the corrections with expectations and their fairly substantial magnitude suggest that they represent real measures of change in case difficulty rather than quasi-random fluctuations.

Table 2. Proportion of Supreme Court Decisions Supporting Civil Liberties Claims, 1946-85 Terms

Natural Court	Terms	Proportion
1	1946-48	49.5
2	1949-52	38.7
3-5	1953-56	57.5
6	1956-57	64.2
7-8	1958-61	57.8
9	1962-64	79.1
10	1965-66	65.2
11	1967-68	75.8
12-13	1969-70	50.3
14	1971-75	48.2
15	1975-80	39.1
16	1981-85	37.2

Note: "Natural Court" refers to the number of the natural court or courts included, counting in sequence from the beginning of the 1946 term. Where a term is included in two natural courts, the dividing line occurred during the term; substantial portions of the 1956 term lie in each natural court, while the 1975 term lies primarily in the fifteenth natural court. Mixed decisions are counted as one-half favorable.

Table 3. Adjustments in Civil Liberties Support Based on Corrections for Pairs of Natural Courts, 1946–85 Terms

Natural Court	Terms	Support Score	Correction from Previous	Cumulative Correction	Adjusted Support
1	1946–48	49.5	.0	.0	49.5
2	1949–52	38.7	−7.3	−7.3	31.4
3-5	1953–56	57.5	−2.0	−9.3	48.2
6	1956–57	64.2	− .5	−9.8	54.4
7-8	1958–61	57.8	+3.5	−6.3	51.5
9	1962–64	79.1	.0	−6.3	72.8
10	1965–66	65.2	+7.2	+ .9	66.1
11	1967–68	75.8	+ .4	+1.3	77.1
12-13	1969–70	50.3	+8.3	+9.6	59.9
14	1971–75	48.2	−7.3	+2.3	50.5
15	1975–80	39.1	+4.1	+6.4	45.5
16	1981–85	37.2	+ .5	+6.9	44.1

Note: Scores are in percentages. "Correction from Previous" is the adjustment from the previous natural court based on the median difference in civil liberties support by justices who served in both natural courts. "Cumulative Correction" is the total adjustments from the first natural court to the court on that line.

The calculated corrections are applied to the Court's support scores in the final column of Table 3. The corrections did not produce any fundamental changes in policy trends (the correlation between old and new support scores is .88), but some important modifications occurred. The conservatism of the late Vinson Court becomes even more pronounced, and the Warren Court of the 1950s appears somewhat less liberal than it had appeared before. The Court of 1967–68, not the Court of 1962–64, now appears the most liberal.

The Burger Court shows the most striking results from the corrections. The gap between the Burger Court and the late Warren Court narrows a bit. More important, the Burger Court now appears to be almost as civil libertarian as the early Warren Court and the early Vinson Court and far more civil libertarian than the late Vinson Court.

These adjustments seem intuitively reasonable. The sharper distinction between the early and late Warren Courts is more consistent with the Court's personnel changes and its dramatic innovations in civil liberties in the 1960s. The Burger Court's standing now matches its mixed doctrinal positions on civil liberties issues. The peak in civil liberties support after Marshall's appointment also makes sense. While these results hardly establish the accuracy of the correction procedure used in the study, they show that it produces realistic results.[7]

Conclusions

I have laid out and applied one method with which to control for issue change in comparing the ideological position of the Supreme Court or of other institutions over time. This method is neither perfect nor definitive, because it incorporates assumptions that oversimplify the Court's reality. But it provides one means to compare policy positions over time, and the results from its application to civil liberties policy are intuitively reasonable.

Quite aside from the value that this specific method may have, the utility of taking issue change into account in analyzing policy change should be clear. Some students of Congress and the Supreme Court have worked to measure and control for the effects of issue change. We need to continue thinking about means to measure issue content that will be applicable to a variety of situations. In this way, we can ease the difficult task of understanding policy change and ultimately of comprehending individual and institutional policymaking behavior.

Notes

I would like to thank Jeannine Sanford for her extraordinary research assistance, Aage Clausen and Herbert Weisberg for their considerable help, and Saul Brenner, Gregory Caldeira, William Jacoby, and Jeffrey Segal for their comments. An earlier version was presented at the 1987 annual meeting of the American Political Science Association, Chicago.

1. Arguably, at any given time two justices might differ in their perceptions of the difficulty of cases before them because one has experienced the evolution of issues in an area longer than the other. The high scalability of cases with varying difficulty in any given period suggests that such differences would be minor. Further, even relatively new justices are likely to come to the Court with sufficient experience in observing and assessing the issues that the Court addresses to perceive evolution that has occurred.

2. The basic stability of the justices' positions is suggested by the high correlations between individual levels of support for civil liberties in adjoining natural courts for justices who served in both periods. Of the 11 natural court-to-natural court correlations, the median is .96.

3. A full list of the coding rules summarized here is available from the author.

4. Freedom of expression claims by the mass media generally were included.

A second analysis was performed in which cases for each period were subjected to Guttman scaling, and those with two or more errors were excluded. This procedure was used to eliminate cases with anomalous voting patterns that may result from multiple dimensions of attitudes. The results were quite similar to those for the main analysis presented in Table 3.

5. Where the breakpoint between natural courts was ambiguous, it was placed between terms if possible. In the combined natural courts, justices were included only if they served during a natural court that constituted a majority of the combined period.

6. The variation in the scores for individual justices that appears in Table 1 exists for other periods as well. The mean deviation in each period ranged from 2.7% to 9.2% (the second highest was 6.6%). Commonly, a single justice was an outlier, increasing the mean deviation substantially. To limit the effects of outliers—who may be interpreted as justices departing the most from the assumptions underlying this method—the median of the scores was used rather than the mean. The variation among individual difference scores underlines the imperfections of the assumptions on which this method is based. The moderate size of the mean deviations is reassuring because it suggests that there is a real central tendency in the difference scores, one that reflects the primacy of issue change as a source of changes in individual voting behavior.

In 4 of the 11 periods, only positive individual difference scores or only negative scores appeared. If we interpret the deviations among scores as reflecting forces other than issue change, such periods are no more consistent with the assumptions underlying this method than are other periods. Such periods differed from the others primarily in the clustering of individual scores at high positive or negative levels rather than near zero, and it is reasonable to assume that the magnitude of issue change will differ considerably from period to period.

7. The pattern of results should allay one reasonable concern about the correction method. It could be argued that external forces move the Court in a liberal or conservative direction during particular periods and that the correction method would simply treat such movement as a change in case content and suppress it. Some of the corrections do reduce the size of the calculated policy change, but others increase it. Thus the correction method appears to be quite independent of the direction of change in the pattern of case outcomes.

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DEFENSE BURDEN AND ECONOMIC GROWTH: UNRAVELING THE TAIWANESE ENIGMA

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Taiwan has displayed a comparatively heavy defense burden and rapid economic growth. Time series data from this case are used to analyze three models of the effects of defense burden on economic growth: the modernization model, the capital formation model, and the export-led growth model. The results indicate that all these models capture parts of the empirical reality, but none can account for all the complexity of this reality. Indeed, in some crucial respects each of the models—based, as they have been primarily, on intercountry comparisons—is contradicted by the Taiwanese time series. I conclude that Taiwan has not so much been able to avoid entirely the trade-offs between defense and growth as to relax these trade-offs. The reasons contributing to this relaxation tend to set the Taiwanese experience apart from the experiences of most other countries.

Compared with most developed, as well as developing, countries, Taiwan has, historically, borne a rather heavy defense burden. Yet it has achieved one of the highest and most sustained growth rates in gross national product (GNP)(at an average annual rate of 8.8% during 1952–85) and in exports (at an average annual rate of 24.0% during 1953–85) in the world. Its savings rate, its inflation rate, and its unemployment rate again rank it among the top performers globally. These accomplishments make Taiwan a rather exceptional case in regard to the generally supposed negative impact of defense burden on economic performances.

I first present three alternative perspectives on the economic effects of a heavy defense burden. The results of the time series analyses on Taiwan are discussed next. The conclusion tries to put Taiwan's experience in a broader cross-national context.

Alternative Expectations

Ever since Russett's (1970) pioneering work, there has been a burgeoning literature on the growth effects of defense spending. In the interest of brevity, I will not attempt a comprehensive review of this literature. This review has been undertaken elsewhere (Chan 1985). Instead, I will extract three strands of arguments (perspectives, if you will) from this literature and seek to determine their respective correspondence with the Taiwanese experience.

The first perspective features the military as a modernizing agent in developing countries. According to Benoit (1973), the negative consequences

AMERICAN POLITICAL SCIENCE REVIEW
VOL. 82 NO. 3 SEPTEMBER 1988

of defense spending on economic growth can be neutralized, and even reversed, by three processes. He contends that the military instills in its members modern skills and attitudes; that capital investment by the military improves a country's basic infrastructure in communication and transportation; and that government spending in general creates mild inflationary pressure, which in turn encourages fuller utilization of existing production capacity.

The second perspective highlights the dampening effects of military expenditures on capital formation, which is in turn considered a key determinant of long-term economic growth (Deger and Smith 1983). A larger percentage of GNP allocated to the defense sector is apt to be financed through a heavier tax burden or bigger government budget deficits or both. Both tend to dampen investment, with taxation playing a more direct role (people have less income to save and invest) and budgetary deficits playing a less direct role (deficit spending abets inflation, which in turn encourages consumption and discourages saving). In the absence of offsetting transfers of foreign resources into a country, the consequent decline in capital formation slows economic growth.

The third perspective stresses the adverse impact of defense expenditures on a country's export competitiveness (Rothschild 1973). It argues that such spending is particularly likely to divert resources (human and capital) from the most dynamic industrial sectors of the economy. These sectors tend to be export-oriented, and export competitiveness is a major determinant of national economic growth. By hampering the innovation and production efficiency of civilian industries that are necessary for successful export drives, a heavy defense burden is seen to bring about deteriorating trade balance, weakening currency, and structural unemployment.

Analysis Results

Although the pertinent times series for Taiwan extend in some cases to the early 1950s, I restrict my analyses to the 1961–85 period. During the years before 1961 (especially the early 1950s), the Kuomintang was struggling quite literally for survival in its last territorial remnant. By 1961, however, political stability and economic recovery had been achieved, so that the subsequent years represent a period of "normalcy."

I rely on the Arms Control and Disarmament Agency's annual *World Military Expenditures and Arms Transfers* for Taiwan's defense figures. All my other data are based on Council for Economic Planning and Development 1986. Generalized least square regression with the Yule-Walker estimation procedure is used for adjusting for autocorrelation in the data with a lag of one year. This procedure takes into account the effects of secular trends, and is appropriate and efficient when the number of observations and the autoregressive parameters are small. The generalized least square results are reported in Table 1.

I discuss first the evidence pertaining to the various causal paths suggested by the modernization model. Equation 1 shows that military spending as a percentage of GNP (MEGNP) has been negatively, rather than positively, associated with the development of Taiwan's industrial infrastructure (TRANSCOM, an index of transportation and communication facilities). This association casts doubts on the contention that capital investment by the military promotes infrastructure development.

Equation 2, however, is more supportive of the modernization model. It indicates that MEGNP has tended to stimulate fuller capacity utilization, which is indexed by the rate of energy production (ENERGYOP, the rate at which energy-generating plants operated as a percentage

Table 1. Generalized Least Square Model Estimates, 1961–85

Equation	Dependent Variable	Independent Variable	*B* Coefficient	T significance	*R* square (%)
Modernization model					
1	TRANSCOM	MEGNP	-173.76	.0242	21.9
2	ENERGYOP	MEGNP	1.2708	.0894	13.1
3	LITERACY	ARMFORCE	- .5997	.0001	80.6
4	BIRTHRATE	ARMFORCE	.0582	.0001	74.6
5	GNPRATE	(FORGNRES − MEGNP)	- .1970	.0001	
		TRANSCOM	- .0024	.0823	
		ENERGYOP	.4936	.0002	
		LITERACY	.4753	.0364	
		BIRTHRATE	9.3072	.0006	
		MEGNP	- .7215	.0134	
		ARMFORCE	- .3140	.0633	91.8
Capital formation model					
6	TAXBURD	MEGNP	- .3441	.0777	14.1
7	BUDBAL	MEGNP	-3,205.5	.0084	28.7
8	PRICE	BUDBAL	.0007	.0024	34.9
9	SAVEGNP	TAXBURD	2.4716	.0002	
		PRICE	- .0088	.8916	50.0
10	CAPFORM	SAVEGNP	.4060	.0339	18.9
11	SAVEGNP	MEGNP	-1.1998	.0945	12.8
12	CAPFORM	MEGNP	-1.6495	.0161	24.6
13	GNPRATE	(FORGNRES + CAPFORM)	- .0023	.1089	11.3
14	GNPRATE	TAXBURD	- .4880	.3442	
		BUDBAL	.0001	.1597	
		PRICE	- .1581	.0701	
		(FORGNRES + CAPFORM)	- .0031	.0819	
		MEGNP	.3321	.5527	49.3
Export-led growth model					
15	EXPRATE	MEGNP	2.1679	.2752	5.6
16	INDEXP	MEGNP	-1.5159	.3166	4.8
17	TRADEBAL	EXPRATE	-449.81	.5958	1.3
18	CURRENCY	TRADEBAL	.000002	.5490	1.7
19	GNPRATE	EXPRATE	.1131	.0125	
		INDEXP	.0045	.9224	
		MEGNP	.2628	.6271	32.1
20	UNEMPLOY	EXPRATE	- .0040	.2735	
		INDEXP	- .0271	.0004	
		GNPRATE	- .0008	.9675	49.2

of their installed capacity). The time series for ENERGYOP has fluctuated within a range between 64.3% (in 1967) and 42.5% (in 1985), indicating the presence of substantial industrial slack or underutilized capacity—a situation that perhaps distinguishes Taiwan from other developing countries. It is this slack that enables MEGNP to stimulate faster GNP growth. In its absence, MEGNP would only overheat the economy without expanding output.

The predicted effects of a large military establishment on the spread of modern skills and attitudes are refuted in Equations 3 and 4. A shrinking ARMFORCE (the number of military personnel per one thousand people) has actually been accompanied by rising literacy and falling birth rate. Both of these overtime relationships are highly significant at the .0001 level.

The multiple regression presented by Equation 5 controls for FORGNRES, or the net transfer of resources (e.g., aid, loan, investment) to or from Taiwan as a percentage of its current GNP. It is hypothesized that the difference between FORGNRES and MEGNP will be positively correlated with GNPRATE. If the former exceeds the latter, growth is stimulated. If the latter exceeds the former, growth is retarded. Incidentally, until 1962 Taiwan received between one-third and one-half of its domestic capital formation from foreign (mainly U.S.) sources. However, more recently it has become a net capital exporter.

In Equation 5, the international flow of resources (FORGNRES), after adjusting for MEGNP, features a significant negative overtime relationship with GNPRATE, indicating that Taiwan's fastest economic growth occurred after the cessation of massive U.S. aid and while it was "graduating" to the status of a capital exporter. Also, both MEGNP (*T* significance = .01) and ARMFORCE (*T* significance = .06) have been negatively associated with the rate of GNP growth. Therefore, the causal path linking MEGNP to GNPRATE through the mediating influence of ENERGYOP is the only proposition advanced by the modernization model that receives empirical support. Whatever positive effect defense burden has had on GNP growth indirectly through fuller capacity utilization seems to have been more than offset by its other direct or indirect negative effects.

It may appear enigmatic that TRANSCOM features a negative overtime relationship and BIRTHRATE a positive one with GNPRATE. Taiwan's economy grew faster during the earlier part of the time series when its industrial infrastructure was less developed and its birthrate was higher than now. Reflecting a process of industrial maturation, the very fast rates of growth of the earlier years have shown a tendency to level off somewhat during the 1980s. This deceleration occurred in the context of continued infrastructural development and mass adoption of modern attitudes (for which BIRTHRATE is used as a surrogate measure). Regarding the impact of the latter two variables on economic growth, there is therefore a tendency for diminishing returns to set in. It is important to reckon with such longitudinal patterns that may be missed and may even be treated as counterintuitive in cross-national analyses.

How well does the capital formation model fare? MEGNP features an unexpected negative association with tax burden (TAXBURD, Equation 6), although it has had the predicted effect on budget balance (BUDBAL, Equation 7). The overtime relationship between budget balance and inflation (PRICE) shows a positive sign (Equation 8) and thus is again contrary to the expectation of the capital formation model. Inflationary pressure in Taiwan appears instead to have been driven primarily by external economic forces (e.g., global recessions, energy crises). In Equation 9, the hypothesized negative impact of TAXBURD on the savings rate (SAVEGNP) is again contradicted, and significantly so, by a positive *B* coefficient, whereas that of PRICE turns out to be insignificant.

But could MEGNP have affected savings and investment directly rather than indirectly through the mediation of TAXBURD, BUDBAL, and PRICE? Equations 11 and 12 indeed show evidence of a direct trade-off; higher MEGNP has been associated with lower SAVEGNP (*T* significance = .09) and CAPFORM (domestic capital formation as a percentage of GDP, *T* significance = .02). Do CAPFORM and FORGNRES jointly contribute to faster GNPRATE? This expectation, unfortunately for the capital formation model, is not borne out by the data. Equation 13 shows that their over-time relationship carries a negative sign.

Incorporating the aforementioned variables in a single regression produces Equation 14. This equation shows that CAPFORM, after adjusting for FORGNRES, bears a slight negative relationship with GNPRATE at a moderately significant level. MEGNP does not appear to influence economic growth directly, whereas PRICE does tend to affect it adversely. To the extent that Taiwan's inflation has been primarily "imported" from abroad, PRICE serves as a proxy measure of the state of the world economy to which this trading nation is highly sensitive.

In summary, MEGNP does appear to have a direct dampening effect on savings and investment, as suggested by the capital formation model. However, bivariate analyses fail to show any significant effects of either CAPFORM or FORGNRES on the rate of economic growth. In the multivariate analysis, the combined effect of CAPFORM and FORGNRES on GNPRATE turns out to be actually negative. Therefore, only one part of the capital formation model, addressing the adverse impact of military spending on investment, receives support from the Taiwanese case. The other part of this model, addressing the linkage between capital formation and economic growth, is not borne out. This is perhaps largely due to Taiwan's recent possession of rather large surplus capital. It has the highest savings rate and the largest foreign reserve (on a per capita basis) in the world.

Contrary to the export-led growth model, Equations 15 and 16 indicate that MEGNP does not provide a good predictor of Taiwan's rate of export expansion (EXPRATE) or of the industrial component of its exports (INDEXP). The evidence is also not strong for the hypothesized linkage between EXPRATE and TRADEBAL (trade balance controlled for GNP) or that between TRADEBAL and CURRENCY (the number of Taiwanese dollars necessary to exchange for one U.S. dollar)(Equations 17 and 18). The latter finding is perhaps not surprising in view of the previously tight governmental regulation of foreign exchanges.

The other part of the export-led growth model—addressing the causal link between export performance on the one hand and economic growth and unemployment on the other hand—does receive some support from the Taiwanese time series. In Equation 19, EXPRATE (although not INDEXP or MEGNP) is a highly significant determinant of GNPRATE. At the same time, INDEXP (but not EXPRATE or GNPRATE) shows up as a key factor influencing the unemployment rate (Equation 20). Consequently, as in the case of the capital formation model, only one part of the causal path suggested by the export-led model is substantiated by the data. Taiwan's economy has been very sensitive to export performance, but military burden has not appeared to be a major determinant of export performance.

Conclusion

Let us review briefly the highlights of the longitudinal evidence presented earlier. Regarding the modernization model, military expenditure has not promoted infrastructural development, although it has apparently stimulated fuller capacity utilization. Contrary to its hypothesis, a large armed force has not contributed to the spread of modern skills and attitudes—or at least not in the way that these concepts have been operationalized in this analysis. The declining size of the Taiwanese armed forces has been accompanied by rising literacy and falling birth rate, thus hardly supporting the view of the military as a key modernization agent.

As suggested by the capital formation model, military spending has curtailed savings and investment rates. However, the latter variables are not strong predictors of the overtime changes in Taiwan's GNP growth. On the other hand, both Taiwan's GNP growth and its unemployment level seem to have been strongly influenced by its export performance. The export-led growth model is deficient, however, because military spending has not had the hypothesized adverse impact on export competitiveness.

Some of the data relationships discovered are in fact less puzzling than they may first appear. Intercountry patterns often disappear and are even reversed when the same variables are examined longitudinally for each country. There may very well be a threshold in the overtime relationships between economic growth on the one hand and processes such as investment in human capital, development of basic infrastructure, accumulation of capital, and promotion of industrial exports on the other hand. Beyond this threshold, the marginal contribution of each additional increment of the latter variables to economic growth may well diminish, thus accounting for the negative and statistically significant coefficients featured by TRANSCOM and (FORGNRES + CAPFORM) in explaining GNPRATE.

Furthermore, for some of the variables examined, there are ceiling effects for either statistical or substantive reasons. With its industrial exports already constituting 93.8% of all exports, there remains less room for Taiwan to improve this percentage further. As another example, with a savings rate that never fell below 25% of gross domestic product since 1970—and was as high as 34.9% at one time—domestic investment opportunities begin to be exhausted and investment attention tends to be shifted to foreign targets. And of course it is far more difficult to duplicate the feat of a 13.9% annual increase in real GNP or a 53.8% annual increase in exports during the more recent years than during the earlier years when the base figures for both were smaller.

Is there any evidence of defense-versus-growth trade-offs? The Taiwanese experience shows some statistical evidence of such trade-offs. As mentioned earlier, military expenditure tends to dampen savings, investment, and infrastructural development. It also tends to stimulate the economy, thereby possibly crowding out civilian production and fueling inflation in a country without industrial slack.

How can we explain these statistical patterns? It seems to me that Taiwan's accomplishments in GNP growth, export expansion, rising physical welfare, and improving income equality are products of a general socioeconomic process in which defense burden plays only a minor and indirect role. Fluctuations in the level of military expenditures and manpower happen to be in most cases the concomitants rather than the determinants of this process.

A search for possible determinants of Taiwan's GNP performance reveals price stability and export rate to be especially important factors. These variables imply that foreign economic environment plays a critical role in affecting the health of Taiwan's economy. This economy has been overwhelmingly externally oriented, with foreign trade amounting to about 85% of its GNP in 1985. Thus the export-led model is indeed correct in assigning paramount importance to trade competitiveness as an explanation of Taiwan's economic performance. However, defense burden seems not to have been a major factor in determining Taiwan's trade competitiveness. Instead, the latter has more to do with improving human capital, product upgrading, commercial adaptability, negotiation skills (e.g., Chan 1987; Yoffie 1983), and a booming world economy during the 1960s.

But even this account is not entirely satisfactory because it begs the question of why the Taiwanese have been able to attain these achievements and to relax—if not entirely avoid—the defense-versus-growth trade-offs. Other countries, with seemingly better domestic endowments and a more conducive foreign environment, have fared less well, as indicated by Taiwan's status as a frequent deviant "overperformer" in cross-national patterns. In order to unravel this puzzle, it seems that one must go beyond the narrow confines of the statistical evidence provided earlier and probe more deeply into the historical context of Taiwan's political economy.

Various historical legacies and timing assume major importance. Japanese colonialism provided Taiwan with a comparatively developed human and industrial infrastructure (e.g., Cumings 1984). It and the Chinese civil war paved the way for a strong state on the island (Amsden 1985; Barrett and Whyte 1982; Chan n.d.; Clark 1987). The massive U.S. aid (coming on the heels of the Korean War, McCarthyism, and Washington's "containment" policy) as well as a booming world economy during the 1960s (when the field of the newly industrializing countries was not so crowded) arrived at very opportune times as the Kuomintang tried first to institute political control and economic recovery and then to launch its export drives. In addition to the U.S. aid (Jacoby 1966), a substantial surplus industrial capacity and a large pool of relatively trained labor helped to lessen the adverse impact of defense burden on the economy. And of course the regime has so far eschewed huge capital outlays to finance indigenous armament industries in any way comparable to the Israelis or even the South Koreans. It has instead chosen to emphasize Taiwan's industrial comparative advantages in indigenous armament production selectively, most notably in the shipbuilding area. These considerations should not be dismissed as ad hoc explanations, but must be included in any valid attempt to unravel the Taiwanese "exception."

Note

Jani Little's help in undertaking the computer analysis for this study is gratefully acknowledged.

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CONTROVERSIES

CONTENDING MODELS OF CABINET STABILITY

A spirited debate has arisen over the best approach to the analysis of the durability of governing coalitions in parliamentary democracies. In this controversy, Kaare Strom enters a number of criticisms of the stochastic modeling approach offered by Eric C. Browne, John P. Frendreis, and Dennis W. Gleiber, and by Claudio Cioffi-Revilla. In turn, Browne and his colleagues join the issue.

Since Riker's (1962) seminal work, game-theoretic (specifically coalition-theoretic) approaches have dominated the study of government coalitions in parliamentary democracies. Riker's *size principle* and competing *policy distance* theories (Axelrod 1970; Swaan 1973) have engendered cross-national studies of cabinet formation, intracoalitional payoff distributions, and government stability (durability). Predicted coalitional properties such as minimum winning status and ideological connectedness figure prominently as explanations of durability (Axelrod 1970; Dodd 1974, 1976; Laver 1974; Mitra 1980). Government stability has also interested institutionalists, whose explanatory models include such variables as party system fragmentation, the number of parties in government and opposition, and ideological conflict levels (Brass 1977; Powell 1982; Sanders and Herman 1977; Strom 1985; Taylor and Herman 1971; Warwick 1979). Frequently, game-theoretic propositions have also been incorporated in such regression models.

In recent years, this game-theoretic and institutionalist literature has come under criticism. Government durability, it is argued, can more fruitfully be understood through stochastic models (Browne, Frendreis, and Gleiber 1984, 1986a, 1986b; Cioffi-Revilla 1984; Midlarsky 1984; Robertson 1983a). Cabinet dissolutions result from generally unpredictable "critical events" that arise during a government's "lifetime." Accordingly, cabinet duration is best explained by models derived from survival or reliability analysis, such as the Poisson model. In this paper I shall critically evaluate this new approach, focusing on three prominent works (Browne, Frendreis, and Gleiber 1984, 1986b and Cioffi-Revilla 1984). Although innovative and methodologically fairly sophisticated, these analyses are misconceived, poorly executed, and ultimately unlikely to advance our understanding of government coalitions. Specifically, the stochastic modelers have (1) misrepresented the game-theoretic tradition and its empirical success, (2) paid insufficient attention to key terms and assumptions in their own models, (3) overinterpreted empirical support for these models, (4) misplaced the focus of the analysis through substantive unfamiliarity or faulty metaphor, and (5) provided a poor alternative to the game-theoretic approach in the development of a cumulative and rigorous science of politics. After addressing these propositions in order, I offer some suggestions for further research on government stability. The critical events perspective may yet prove its value, but only within a much stronger micropolitical theory.

The Shortcomings of Coalition Theory

The stochastic modelers have leveled three main criticisms against coalition-theoretic explanations of cabinet duration, namely, that they (1) incorporate unrealistic premises and are therefore unpromising (Browne, Frendreis, and Gleiber 1984, 173); (2) are deterministic (Browne, Frendreis, and Gleiber 1984, 1986b; Cioffi-Revilla 1984); and (3) have been empirically unsuccessful (Browne, Frendreis, and Gleiber 1986b, 630-31). On this basis, the authors argue for a "very different research program" (Browne, Frendreis, and Gleiber 1986b, 634). However, these criticisms are not particularly telling. Even if they were, they would not justify abandonment of the game-theoretic approach.

The first criticism is the most interesting. Browne, Frendreis, and Gleiber accuse coalition theory of simplistic assumptions about party preferences, such as positing a stable payoff function for an entire legislative term (1984, 173). Similarly inappropriate is the assumption that successive government formations are mutually independent events (p. 174). While these criticisms are valid and reasonable as far as they go, they are neither novel nor fatal to coalition theory. Coalition theorists have in fact relaxed such assumptions as the mutual independence of cabinet formations (Bueno de Mesquita 1975; Laver 1974). And while most empirical models have been static in the sense of predicting government formation and maintenance from a conception of fixed equilibria, such stipulations need not be made (see Bueno de Mesquita and Niemi 1984; Laver and Underhill 1982). Thus, the critics have demonstrated that coalition theory is frequently simplistic, but not that it is incorrigible. Similar criticisms have in fact been made within the game-theoretic tradition (Laver 1986; Winden 1984).

The charge of determinism is much less warranted. Regression models employed in empirical coalition-theoretic studies are deterministic in the sense of yielding point predictions of cabinet duration, but capture stochastic elements of the process through error terms. However, game-theoretic durability studies are not deterministic in the strict *theoretical* sense of specifying the sufficient conditions for government dissolution. While government *formation* studies are typically deterministic in predicting specific (and occasionally unique) solutions, this is not true of similar studies of cabinet *durability*. There is no deductive theoretical argument that cabinet duration is precisely determined by the structure of the formation game. Rather, the argument tends to be cast in probabilistic terms (e.g., Dodd 1976). Cioffi-Revilla misrepresents the competition when he presents as the only alternative to his analysis a deterministic model in which cabinet duration is "entirely determined and exactly predictable" from the set of independent variables (1984, 334). Obviously, this is a hopeless task on which no sensible coalition theorist would embark. However, theoretically interesting explanations need not meet such heroic requirements.

The third and final criticism of the extant literature is directed against its empirical success. Cioffi-Revilla's critique on this point is unsatisfactory, since he presents such a caricature of the "deterministic" approach.[1] Browne, Frendreis, and Gleiber more soberly contend that existing models of cabinet durability generally explain only 20-30% of the variance. This, we are told, is disappointing, given more than 10 years of empirical research. Moreover, traditional studies have allegedly reached an upper limit of predictability, partly because of the misguided assumption that each cabinet embodies a certain predetermined durability (1986a, 94). This limitation is allegedly also due to the specification of the regression models, in which the number of independent variables approaches the number of cases (1986b, 630).

This critique can be countered in several ways. First, these critics place excessive emphasis on the coefficient of determination (R^2), which may be affected by a variety of factors of little theoretical interest (see King 1986). Second, several empirical studies have in fact exceeded the 20–30% barrier in predictive accuracy. Powell (1982, 149) reports that two variables alone explain 50% of the variance in cabinet durability (see also Strom 1985; Warwick 1979).[2] The more powerful of these two predictors is minimum winning status. So the bottle that is half empty to Browne, Frendreis, and Gleiber, may be half full to many other social scientists, who would happily accept such explanatory power. Indeed, this is precisely the assessment made by the otherwise critical Laver (1986, 32–33). Finally, it is premature to conclude that an upper limit of explanatory power has been reached, particularly since the number of cases in the typical study runs in the hundreds and the number of independent variables is fewer than five. Thus, a degrees-of-freedom problem is not evident.[3]

Events and Cabinets in the Stochastic Models

Let us now consider the stochastic models presented by these critics of the game-theoretic tradition. Their main flaw is a failure to illuminate the political decisions that cause a change of government. The explication of how and why critical events effect changes in political alignments is inadequate. Simply linking government dissolutions to extraneous events does not *explain* the former any more than precipitation explains the investment decisions of farmers. While there may be an empirical (and even causal) relationship, explanation requires a linkage to our understanding of human behavior.

Cioffi-Revilla does not explicate the micropolitical connections between critical events and government dissolutions. After citing a few examples, he simply asserts that a multitude of causes can cause the loss of pivotal votes in parliament (1984, 321). He also claims, unobjectionably, that political shocks and crises "probably come from the characteristics of the Italian political system as a whole" (p. 334). Nowhere is there a micropolitical interpretation, much less a theory, of the relationship between critical events and government resignations. While Browne, Frendreis, and Gleiber recognize its importance, their micropolitical argument is neither consistent nor persuasive. In a recent article, they portray cabinet stability as resulting from decision processes (1986b, 637). A critical event represents uncertainty in a government's environment, a "demand" to which it must "respond." But this conception is contradicted in their earlier and more extensive discussion, where events sometimes lead directly to cabinet dissolution, affecting neither decision-making structure nor actor preferences (1984, 184). The authors have in mind events such as constitutionally mandated elections or the death of a prime minister. But such events render their entire conception of government resignations unilluminating. Dissolution is described as "the response to a demand that actors will not or cannot satisfy" (1984, 181). If a dissolution were in fact caused by the premier's death, then this description would be meaningless.

Browne, Frendreis, and Gleiber suffer from attempts to generalize across very heterogeneous government dissolutions. Some of these can fruitfully be analyzed in terms of decisions, demands, and satisfactions; others cannot. The authors face this predicament because they neither develop their micropolitical argument well enough nor define their terms adequately. Curiously, there is no serious attempt anywhere in this literature to define either *critical event* or *cabinet dissolution.* These are, after all, the key terms in the stochastic models. Cioffi-Revilla is content to equate critical events with random political shocks and crises, "having numerous manifestations, such as scandals, parliamentary defeats, pivotal defections, and other occurrences" (1984, 334). Browne, Frendreis, and Gleiber admit to having "at present little systematic knowledge of such events" (1984, 185). Like Cioffi-Revilla, they are confined to unsystematic exemplification (1986b, 633). Note also that critical events are not empirically observed but simply inferred from the model. The basic causal factor in the stochastic models, then, is *undefined* and *unobserved.*

Browne, Frendreis, and Gleiber (1986b) test their Poisson model in separate analyses of 12 parliamentary democracies between 1945 and 1980. Their hypotheses are supported for four countries, but must be rejected for the remaining eight. This result becomes even less impressive when one considers the low degrees of freedom, which dilute the power of their chi-square test. Furthermore, the authors are certainly wrong in presenting their empirical model as a test of alternative hypotheses (p. 641). First, the fit of the Poisson model is never compared to that of any other. Second, it is theoretically possible to specify an infinite number of other models (some presumably substantively meaningful) with equal or better fits. The proof of the pudding clearly cannot rest here.

A further problem is that these Poisson models are based on the unrealistic assumptions of *homogeneity* and the absence of *contagion* (see Davis, Duncan, and Siverson 1978). Let us examine these problems with reference to Cioffi-Revilla's analysis. The author interprets his results as evidence of a common Poisson process operating on the durability of all Italian governments. However, the aggregate results could just as well mask a pattern of very heterogeneous durabilities across cabinets. Heterogeneities may occur over types of cabinets or over time. Thus the most short-lived governments may also be *monocolori* rather than majority governments, ideologically "open" rather than connected, or those formed during the crisis of a particular coalition formula (e.g., the Center-Left). Indeed, there appears to be empirical support for each of these possibilities (see, e.g., Marradi 1982). Also, durability may vary cyclically, with governments formed very late in a parliamentary term less durable than those formed just after elections (see Sanders and Herman 1977). A Poisson model may or may not be applicable for each subclass of governments, but the aggregate model will tell us little in case of significant heterogeneity. Moreover, *contagion* would be present if the duration of one government depended on that of its predecessor. Again, this possibility should hardly be overlooked. Yet Cioffi-Revilla provides no control for any of these potential problems.[4]

Governments Misconceived

The definition of a government is neither obvious nor inconsequential (see, e.g., Dodd 1976, 121–23; Harmel and Robertson 1986; Hurwitz 1971; Lijphart 1984; Sanders and Herman 1977). After careful review of five definitions, Lijphart (1984) concludes in favor of using changes in party composition as the sole criterion of a cabinet change. Dodd (1976) reaches the same conclusion. Other scholars advance other criteria (Robertson 1983a, 1983b; Sanders and Herman 1977). Cioffi-Revilla and Browne, Frendreis, and Gleiber neither specify nor justify theirs. One may excuse Cioffi-Revilla's lack of explicit criteria since he does not face the problem of cross-national equivalence. Browne, Frendreis, and Gleiber's inattention is more glaring, especially as they actually cite (1986b, 646) Lijphart's instructive article.

Operationally, Cioffi-Revilla seems to rely on political conventions in counting Italian governments; whereas Browne, Frendreis, and Gleiber appear to count a new government with every change in prime minister, party composition, or electoral period. These definitions seem to yield identical results, with more government resignations and far lower cabinet durabilities than the Dodd-Lijphart criteria. In practice, the operational criteria adopted by the stochastic modelers skew their results by introducing substantial amounts of "noise." Recall that a change of government is recorded when the prime minister dies, or when a snap election returns the incumbents to power. This operational definition should in itself favor models of dissolutions as caused by random events and disfavor explanations based on rational party behavior. In fact Lijphart (1984) demonstrates that cabinet durability is significantly more strongly related to such factors as minimum winning status, the number of parties in government, and party system fractionalization under his definition than under the alternative chosen by the stochastic modelers.

Political or academic conventions specifying when a government begins or ends are arbitrary, and operational definitions must be justified by the research problem at hand. The failure of the stochastic modelers to define their dependent variable accordingly has serious ramifications for the underlying theoretical purpose. Government transitions are portrayed as objectively defined phenomena of intrinsic interest to political scientists. But this is not necessarily so. The death of a prime minister need not interest political scientists, unless this event affects the operations or output of political institutions. Both Cioffi-Revilla (1984, 318–19) and Browne, Frendreis, and Gleiber (1984, 169) stress the significance of cabinet durability for political stability. But if systemic stability is the fundamental concern, it appears incongruous to treat, for example, resignations due to the ill health of a prime minister as equivalent to the downfall in 1960 of Ferdinando Tambroni's Italian government in the face of impending civil war. Transitions that are purely technical or involve only minor personnel changes have very different implications for stability from those that represent changes in the set of governing parties (see Dodd 1976, 122; Strom 1985). The same distinction is central if we seek to understand the strategic behavior of political parties. The former is an explicit concern of the stochastic modelers; the latter is common to much of the literature in this field.

The stochastic modelers thus define their cases inappropriately for their theoretical concerns. They also occasionally infuse parliamentary governments with too much life. Indeed, biological metaphors are pervasive. Browne, Frendreis, and Gleiber describe governments as existing in a world that occasionally produces "life-threatening events" (1984, 182), and attributes of the coalitional payoff structure are likened to "protective armor" (1984, 178). In the words of Cioffi-Revilla, "Italian cabinet governments do not age, and they do not suffer political wear; so long as they are in power they are as good as new" (1984, 336). These descriptions suggest that governments can be understood as living organisms or mechanical objects, which, of course, they are not. The metaphors are understandable, since social scientists have largely borrowed stochastic models from biology and engineering. However, uncritical application of such models is not without its hazards. The distinctive requirement of social science is to understand human intention (Elster 1983), which calls for theoretical care in the adaptation of models borrowed from disciplines where intentional explanation is less appropriate.

Scientific Progress?

The fundamental question concerning the stochastic models of government dissolution is to what degree they contribute to a political science better capable of explaining cabinet stability and related phenomena. Progress in political science depends on the development of research programs in the discipline (Ball 1987; Lakatos 1970). In the Lakatosian view, research programs consist of a "hard core" of fundamental propositions or axioms and a "protective belt" of auxiliary hypotheses (see also Laudan 1977). Criticism within a research program should be directed against its protective belt, and scientific progress results if modifications in the protective belt lead to greater corroborated empirical content (Lakatos 1970, 118). Falsification is no simple matter. According to Lakatos, "there is no falsification before the emergence of a better theory" (Lakatos 1970, 119). Since legitimate scientific criticism is directed only against auxiliary hypotheses, it is and should be very difficult to reject a promising research program.

The game-theoretic study of government durability is part of one of the most ambitious research programs in the social sciences. Although this research program has been most successfully developed in economics, it has increasingly stimulated scholarship throughout political science and particularly in the study of voters, parties, and their coalitional behavior (Riker 1983). The stochastic modelers essentially abandon this research program in their study of government stability. Cioffi-Revilla makes no attempt to build on the game-theoretic tradition. To their credit, Browne, Frendreis, and Gleiber do present such an effort, but their discussion is scattered and theoretically underdeveloped. It also contrasts with their eagerness to depict their work as a radical departure from the game-theoretic tradition (e.g., 1986b, 634) and their general indictment of the existing literature.

Abandonment of the rational choice tradition in studies of government durability should be exceedingly well justified. The stochastic modelers clearly cannot justify such a radical departure. Their critique of the existing literature is directed primarily against auxiliary hypotheses and methods. While this is entirely legitimate, it does not add up to a case against the entire research tradition, unless (1) the latter is incapable of incorporating progressive modifications and (2) a superior alternative exists.

Clearly, a superior alternative has *not* been presented. The stochastic models reviewed are both ill-conceived and unilluminating. There is no convincing micropolitical connection between critical events and cabinet dissolutions. No theoretical definition of the dependent variable is provided, and the operationalization is ill suited to the purported theoretical concerns. The independent variable is neither defined nor observed. The empirical models are built on dubious assumptions and yield inconsistent results. Finally, the *predictive* power of the stochastic models is poor. For example, Cioffi-Revilla's model gives us no basis for predicting differences in durability between any two Italian cabinets.

Also, there is promise of a multitude of progressive modifications in the protective belt of the game-theoretic tradition. Game-theoretic analyses of government durability have tended to rely on exceedingly simple assumption about actors, payoff functions, and institutional environments. Despite these restrictions and the limitations of the dominant solution concept (Hardin 1976; Ordeshook 1986, 408–14), empirical tests have actually produced promising results. There is good reason to try to improve these results by relaxing auxiliary assumptions. Most importantly, game-theoretic models of cabinet durability need to become less static. Dynamic elements can most easily be incorporated by allowing utility functions to vary over time. For example, payoffs to political parties may perfectly well change over a parliamentary term. As elections draw close, the value of short-term office holding declines, and electoral considerations become paramount. Also, different parties pursue different kinds and mixes of benefits. Some parties are more office-motivated, others more policy-motivated, and variation may occur within as well as between countries (Budge and Laver 1986). Hence, the constant-sum assumption, on which the size principle is based, is not always applicable. Coalition theory must also incorporate temporal or hierarchical interdependence between governments. Such interdependencies can be formulated as supergames or nested games (see Tsebelis 1987). It is unduly restrictive to view each government formation or dissolution as independent from preceding or successive ones (see Bueno de Mesquita 1975; Franklin and Mackie 1983). Coalition studies may also be based on noncooperative rather than cooperative game theory. New and richer models require greater institutional sensitivity than has often been exhibited by coalition theorists.

In modifying coalition theory, there is something to be learned from the critical events perspective. Coalition theorists have generally paid little attention to the circumstances under which governments resign. However, it does make sense to think of events as keys to cabinet resignations. After all, if nothing happened, there would be no reason to dissolve the government.[5] But an adequate critical events theory of government duration has to meet the following criteria: events must (1) enter the utility calculations of the players in the governmental game; (2) be defined and observed independently of government resignations; and (3) be allowed to vary in their (a) exogeneity and (b) sufficiency as causes of government resignations. The sufficiency problem is recognized by the stochastic modelers, but their assumption of exogeneity is more serious. The events that cause governments to fall are assumed to be uncontrolled and random. In reality, however, such events (e.g., embarrassing publicity) are frequently engineered, or at least affected, by players in the game (i.e., parties inside or outside the government). But if critical events are sometimes endogenous, the Poisson model may be fundamentally inappropriate.

Conclusions

The new stochastic models of government durability are disappointing and inadequate. They are too method-driven and constitute exercises in model-fitting rather than explanation. The most serious problem with the stochastic models lies in their relationship to the game-theoretic research tradition. Stochastic modelers—especially Cioffi-Revilla—present no credible way to integrate their work within this research program. This is not to say that stochastic models have nothing to offer the student of government stability, or that coalition theory should rest on its laurels. Game-theoretic analyses commonly rely on simplistic auxiliary assumptions, which Browne, Frendreis, and Gleiber deserve credit for challenging. Yet their rush to falsification is premature at best. As Lakatos has argued, we should be tolerant in criticism and patient in falsification. Flaws in auxiliary hypotheses do not justify abandonment of one of the most rigorous and comprehensive research programs in political science.

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For some years we have shared with Kaare Strom a concern for explaining various processes associated with cabinet government. Indeed, our work is well known to him since we participated jointly on an APSA panel in 1982, where our stochastic model was first introduced. We also both published papers on the topic in a symposium issue of *Comparative Political Studies* in 1984, and, most recently, our work in the area appeared together with his in a special issue of *Legislative Studies Quarterly* (Frendreis, Gleiber, and Browne 1986; Strom 1986). His current reading and criticism of our approach is surprising in light of this shared history and our own efforts at cumulative theory development, as the following quotation shows:

> The immediate theoretical task is to construct a working model of governmental life-cycle which incorporates the findings of both the structural attributes and random events models. We have noted above the complementary quality of these research findings. They suggest that the next theoretical advance in this area should be the formulation of a model which identifies the ways in which political events trigger changes in the ability of governments to continue in office. As a predictive tool, such a model would describe the conditions under which governments fall, even where the appearance of these conditions is not predictable very long in advance. The two phases of research on governmental duration have respectively stressed the static and dynamic elements of the process. The next phase will incorporate and integrate these elements. (Frendreis, Gleiber, and Browne 1986, 626)

As is apparent, we do not argue that the work that preceded ours is invalid or that a decision-theoretic approach should be rejected.[6] Rather, we see our work and that of Cioffi-Revilla (1984) as presenting an augmented conceptual and methodological specification of the stability problem. We are pleased to have this opportunity to discuss some recent developments and unresolved questions in this field. Although we disagree with much that Strom has said, we do agree that this is an area where the research agenda is full and the certainties are few.

In his rush to discredit this recent work and rehabilitate the research agenda that preceded ours, Strom finds himself defending standard conceptualizations of the problems of cabinet government that he elsewhere criticizes (1986, 583). We see emerging in his critique a new intellectual history that both misrepresents the relationship of past work to game theory and misunderstands the new research agenda that our work suggests. In both instances the problem seems to emanate from interpretative differences regarding investigative procedures that support the normal science of coalition theory. Since we feel that the most important questions facing this field are still ones of theory development, however, we spend the bulk of our space discussing the theoretical issues highlighted by the recent studies. Inasmuch as an appreciation of this recent work requires its fair representation, we begin by briefly identifying some areas in which we feel Strom has presented distorted or erroneous versions of our findings and conclusions.

Strom's Misrepresentations

Strom criticizes our attention to uncertainty and our conceptualization of the appropriate universe of analysis and responds to our critique of the limited predictive power of the structural attributes approach. While Strom is correct in identifying game theory as an important approach to the study of political coalitions, his comments should not be taken to mean that it is a necessary, or the only, approach that has proven useful to researchers.[7] More than one theory and numerous empirical testing methods have been used in explaining the many aspects of cabinet government and coalition processes. His and ours are just two of these.

Strom's presentation is a substantially distorted description of our criticisms of one of the existing research traditions investigating the problem of cabinet stability, which we have variously termed the "deterministic" (Browne, Frendreis, and Gleiber 1984, 1986b) or "structural attributes" approach (Frendreis, Gleiber, and Browne 1986). Central to the structural attributes approach is an implicit assumption that governments more likely to form are also likely to endure longer. Our position has been that this understanding should be expanded to incorporate the idea that a government is brought down by some "event," the *timing* of which is not necessarily related to those factors (attributes)—such as fractionalization or minimal winning status—that other work has identified as being relevant to the process of government *formation*. Throughout our work we have argued the inappropriateness of the traditional assumption that the structural and strategic conditions existing at the time of governmental formation are directly related to the length of time the government is expected to survive. (In other words, these structural attributes do not determine the timing of the event that marks the government's dissolution.) Our labeling of this as deterministic, in the sense of searching for the determinants of governmental duration in the characteristics of the government, has not ever before been confused with deterministic (versus probabilistic) predictive ability. Our replacement of this term with "structural attributes approach" (Frendreis, Gleiber, and Browne 1986) removes any cause for misunderstanding.

Our basic argument remains that while structural attributes may establish the baseline of inherent cohesion of a government (its *durability*, in our usage), they should be unrelated to the length of its historical tenure (its duration) unless there is a causal linkage between these structural attributes and the timing of the events that bring about governmental dissolution. The linkage we suggest depends on the government's dynamic response to events in its environment, the changing of the characteristics of the government over time, or the change in the perception of actors as to the meaning of those characteristics to the problem of continuation in power. Given this complexity, a theory of government stability must specify a model, the statistical testing of which depends critically upon a proper evaluation of the error term, for instance, partitioning the error term of regression-based empirical tests into systematic (though stochastic) effects and the purely random component. We consider reports of the multiple coefficient of determination in the extant literature on cabinet stability to be measures of the information about duration that is carried by various combinations of posited structural attributes. While partial regression coefficients may be useful in estimating the magnitude of relational components in such models, the coefficient of determination is the only measure that describes the whole of the relationship. The persistence of large error terms in these analyses, however, encourages further exploration of the nature of error variance in these models.

The theoretical argument we have advanced in our work has been that the duration of cabinets is appropriately specified by an accounting of the process that results in the dissolution of cabinets. In our treatment, the major theoretical element (admittedly an underspecified, hypothetical construct) has been the postulated occurrence of some "event" (a terminal event) considered sufficient to cause the downfall of a government.[8] We hypothesize that the appearance of such an event over the tenure of a cabinet is random as it is oriented in time and, by extension, that the distribution of such events over some set of cabinets (the set of cabinet durations) is also random with respect to timing. This conceptualization of the downfall process runs counter to the specification of current structural attributes models in which cabinet duration is assumed to be causally determined by an attribute set (e.g., cabinet fractionalization, majority-minority status, etc.)—institutional qualities of governments that either preexist or come into existence with the formation of a cabinet. In structural attributes models, then, these qualities (collectively, cabinet durability) are expected to determine cabinet duration. But if we may conceive duration as determined by downfall, and if downfall is determined by the production of terminal events, then for structural attributes to provide a causal explanation of cabinet duration they must also determine the schedule upon which these events occur. Our specification of an events model (1986b) seeks, indirectly, to examine this issue by testing the null hypothesis that the distribution of cabinet downfalls (durations) is stochastic.[9]

In surveying the reported findings of attributes research on cabinet duration, we have concluded that structural attributes models have likely approached an upper limit of predictability at about 30% of the variance (in duration). Strom seeks to refute this conclusion by citing the results of three studies (Powell 1982; Strom 1985; and Warwick 1979). We regard Strom's report of *R*-squared at .305 and .319 in his own analysis to be unilluminating on the issue. It is the other authors who report substantially higher coefficients. While their methodological decisions are thoroughly consistent with their research objectives (e.g., see Browne, Frendreis, and Gleiber 1984, 1986b for our discussion of Warwick), those same decisions render their empirical results noncomparable with respect to the remainder of duration studies that, like our own work, utilize the universe of governments and weight each government equally. Warwick's and Powell's procedures differ from the other similar studies in that they select cases nonrandomly from the universe of all forming cabinets. Their case selection procedure (and Powell's weighting scheme for governments) serves implicitly to specify additional explanatory attributes (or expunge cases explained by unincluded variables). Such decisions are better made explicitly in a fully specified model. This kind of specification error creates the possibility of significant estimation bias. We consider the attenuation of the relevant universe as the likely explanation of the increase in duration variance explained in these studies, and, therefore, as dubious evidence for optimistic claims regarding the ability of the attributes of cabinet governments to explain (eventually) cabinet duration adequately for the relevant universe. Indeed, an examination of Warwick's alternative models in his Tables 1 and 2 (1979) indicates that even the variables identified as carrying information about duration are redundant to that task. Nor does traditional game-theoretic research offer additional attributes for inclusion.

A final distortion involves Strom's allegation that we have misconceived the basic unit of analysis—specifically, the criteria that determine a change of government (a downfall). The basic charge is that (1) we offer *no definition of* cabinet change (downfall), and (2) *our definition* of cabinet change (downfall) is unfruitful. This contradiction notwithstanding, we believe that neither of these observations is warranted. In a work not cited by him, two of us have discussed the definitional issue in detail (Browne, Gleiber, and Mashoba 1984); and in the articles he does cite, we believe our decision rule is clear. We have considered a downfall to occur whenever there is a change of prime ministers, an augmentation (or diminution) of the number of coalition partners, an accepted cabinet resignation, or the holding of an election (necessitating a new investiture vote). We have also published a complete listing of the 93 Scandinavian governments in our data set, along with their dates of formation and dissolution (Browne, Frendreis, and Gleiber 1986a).[10]

Respecting our decision to include in our analysis all cabinets (save "caretakers") that formed in a country during a particular time period, we believe that the research question requires an accounting of the stability of cabinet governments, and not only those governments that some researchers believe are "interesting" (e.g., those whose tenure is characterized by interparty conflict among coalition partners). Restricting analysis to some such subset of cases is not worthless, for it does allow examination of a particular kind of instability. However, to assert that this is the only kind of (in)stability that is worthy of investigation is to depreciate (to zero) an understanding of other systemic sources of instability and the impact that such processes may have on the decisions of relevant actors. In short, consideration of the entire universe of cabinet formations and dissolutions allows the modeling of the system that political actors confront and characterizes the uncertainty under which they operate. This implies that cabinets have greater or lesser vulnerability to disruptive events during their tenure, increasing or decreasing the probability of dissolution. Restrictive criteria in the selection of cases may be an appropriate tool where the research intent is to demonstrate the operation of certain processes in a "controlled test"; it is less useful—and in fact systematically misleading—if the goal is to explain dissolution and identify the factors that precipitate it in the observed population.[11]

The Research Issues

We shall take up what is perhaps the most crucial of Strom's points, namely, the evaluation of the merit of an events-based approach for understanding stability processes in cabinet governments. We begin by addressing two issues raised by Strom: the relationship of our work to the game-theoretic research tradition on the problem and our supposed failure to provide an adequate "micropolitical" theory to support the stochastic treatment we have given to cabinet stability. After this, we turn to a direct discussion of the potential value of an events-based approach.

We are frankly amazed by Strom's notion that our work has sought to discredit and banish game theory from the arena of coalition research. To the contrary, we have repeatedly argued that cabinet coalition behavior should be ultimately approached as a problem of rational decision making.[12] We assume that our critical survey of research on the problem of cabinet stability (especially in our 1984 article) has unwittingly contributed to Strom's confusion. There we were attempting to uncover the implications of existing coalition models (especially those of Riker [1962] and Axelrod [1970]) for the question of the continuation of a coalition through time. Our conclusion was that these models are essentially time-independent and that this simplification, while allowing for more tractable analyses of the coalition formation problem, presently impedes the application of this work to studies of the persistence of cabinets beyond the point of formation. Later in the same article we observed that uncertainties regarding the persistence of cabinets through time may have implications for modeling the process of coalition formation, and we proposed a noncooperative bargaining model (a game of fair division) as promising (precisely the class of models, Strom notes with approval, currently under investigation by others).

The more serious question, however, concerns how the several related processes associated with coalition behavior have been developed in the literature. Analyses of cabinet coalition formation and payoff allocation have been developed, in the main, from models of *n*-person cooperative games. The problem of cabinet stability, however, has developed from an entirely separate research tradition, beginning with the institutionalist observations of A. Lawrence Lowell (1896) and extending through studies of whole societies (e.g., Almond, Flanagan, and Mundt 1973). Thus, while empirical work on formation and payoff allocations has sought to test propositions and implications deduced from formal models, contemporary work on cabinet stability has sought to relate favored systemic attributes with a surrogate stability measure (typically, cabinet duration). The recent structural attributes research that begins with Taylor and Herman (1971) and extends through Warwick (1979) relies on variables identified by game and coalition theory (e.g., minimal winning status) but is not itself characterized by the formal or deductive methodology associated with game theory. Strom's characterization of the recent stability research as being part of the game-theoretic tradition of coalition studies, then, is simply wrong. More than that, it is mischievous insofar as we are asked to imbue this research with the considerable explanatory power that is characteristic of deductive analytic structures (e.g., game-theoretic models).

Paradoxically, one of the main faults we have found with the structural attributes approach has been its failure to locate the phenomenon of interest (extent of cabinet duration) in a "micropolitical" theory. Rather than specify models of a decision process involving rational behavior by human or party actors, this approach instead proposes that cabinet duration is determined by several systemic attributes that (mostly) are descriptors of institutional parameters within which cabinets function (e.g., majority-minority coalition status, number of party actors, and ideological cleavage in a cabinet). Since the unit of analysis in these studies is the government, we see the model investigating relationships at a level far removed from the decisional behavior of individuals or parties. In fact, no claims regarding individual or party behavior emanate from these studies. In contrast, we have clearly specified a preliminary (and very simple) model of cabinet decision making that is relevant to the problem.[13] Since in our model the downfall of cabinets is attributed to the occurrence of terminal events, the production of such events occupies a central place in any events model of cabinet stability. This does not mean that there is no mediation by political actors; rather, events drive the process. Still unresolved, however, is any theoretical development of the idea of "events processes." Lacking such understanding, we have nevertheless posited their existence and assumed the timing of their occurrences to be uncertain. By extension, the duration of cabinets is uncertain, and thus may be treated stochastically. In our model we have represented the uncertain timing of terminal events by specifying that the duration of cabinets is a random variable with a Poisson-type (negative exponential) distribution.[14]

We have not proposed a model of cabinet stability that is either claimed or intended to be complete in the sense of providing a fully adequate explanation of the processes that govern the downfall of cabinets. The simplicity of our model is for us a necessary first step in developing a new approach to our research problem. Its main departure from the earlier work is to conceive the tenure of a cabinet as a continuous decision problem confronting cabinet actors. At present our concept of events (demands from the environment) is confounded by its inclusion of objective, perceived, and anticipated environmental phenomena. Thus, "event" is at best a hypothetical construct and at worst a positive heuristic not unlike rationality, utility, or perfect information concepts in the core propositions of the touted game theory. Specifically, the tenure of cabinets is conceived as a series of actor decisions regarding the continuing existence of their cabinet, extending through time from formation. The model assumes that over this period, actors are confronted with a series of "events," one of which (a terminal event) will impose a demand that one or more actors will not or cannot satisfy. When this occurs, there is cabinet dissolution.

The test of our stochastic model (Browne, Frendreis, and Gleiber 1986b) was intended solely to establish the plausibility of our conception of the dissolution process, not to provide a predictive model capable of explaining this process, as Strom seems to believe. Indeed, it should have been evident that our model of the dissolution process is perfectly null; it establishes (counter to structural attributes models) an expectation that the continuing existence of cabinets is not systematically influenced by factors in their environment. In other words, the model assumes that if a cabinet exists at any point in time (e.g., on a particular day), the probability that it will exist at the immediately succeeding point in time is the same regardless of whatever point in time is chosen. Thus the particular timing of a cabinet dissolution is uncertain, in fact perfectly uncertain, leaving the expectation that a given cabinet may fall at any time at all.

Our model was tested by a comparison of observed cabinet durations and the hypothesized negative exponential distribution. Here the expectations of the model are "confirmed" if we fail to reject the null hypothesis that cabinet duration is a random variable with a negative exponential distribution.[15] Drawing on duration data from 12 postwar parliamentary democracies, the results supported an inference based on the failure to reject the null in four countries (Belgium, Finland, Israel, Italy); while for the remaining eight, the probability estimates were of such magnitude as to prevent any confident inference with respect to the hypothesis.[16]

Among other things, Strom contends that the empirical test we conducted is merely an exercise in curve fitting and a premature and misleading "rush to falsification." We reject his apparent view that the presentation of anomalous evidence somehow lies outside valid scientific procedure. Strom's appeal to the authority of Lakatos notwithstanding, scientific progress proceeds from raising questions and generating new hypotheses, even when the first steps in that direction are less fully supported by a long-standing theoretical tradition.[17]

The fundamental concern, however, should not be whether we are sufficiently respectful of some theoretical icon but rather whether or not our depiction of the coalition dissolution process is accurate. As the opening quotation indicates, we believe our work complements the earlier tradition, in effect providing a necessary redirection for researchers interested in this problem. We do not question the validity of the findings reported from the analyses relating structural attributes with duration. We tentatively accept, furthermore, the interpretation that systemic attributes may furnish some cabinets with a quality of inherent stability (or durability). It is rather the effectiveness of this quality in preventing (or delaying) the downfall of cabinets that is at issue. The fact that there is correlational evidence to suggest some (rather weak) effect has led us to conclude that "cabinet duration may be expected to be distributed around a durability-determined central point, but with a large average deviation" (Browne, Frendreis, and Gleiber 1986b, 634). Thus, if we are correct in assuming that events are the causative agents of cabinet downfall, the inherent stability of cabinets, furnished by particular attributes, should induce a systematic effect in the timing of cabinet dissolutions. Indeed, it appears that some such effect may have been observed in those eight countries where our test results did not support an inference of failure to reject the null. In the remaining four countries, however, the pattern of cabinet dissolutions supports an inference that the durability attributes of their cabinets were ineffective in impeding the impact of randomly timed terminal events. Indeed, as we have reported, the goodness of fit of the structural attributes and random events models are inversely related across systems (Frendreis, Gleiber, and Browne 1986; Browne, Frendreis, and Gleiber 1986b).

In summary, existing structural attributes models typically report the existence of a positive linear relationship between systemic attributes and cabinet duration, but with a substantial amount of residual error. Attributes researchers have tended to deal with the problem presented by a large error term in two ways. First, they have introduced some new attribute variables or sought new measurements of existing ones in hopes of explaining additional variance. Second, they have decreased the size of the error term through decisions relating to the selection of cases. (Indeed, Strom has criticized us for failure to do this by including in our data set cabinets that do not terminate for what he considers reasons related to political conflict.) Our events approach to the problem takes issue with the presumption that the identification of new attributes (or measurements) is likely to result in a substantial reduction of the remaining error variance in regression models of the process. Rather, we believe that an essential component of the process that causes the particular timing of cabinet downfalls is intrinsically stochastic. For some countries systemic attributes appear to have no appreciable impact on the timing of cabinet dissolutions. In the remaining countries, attributes may act in such a way as to prolong (or foreshorten) the duration of cabinets from what would be expected of a perfectly random downfall pattern. Even here, however, variability over the range of probability estimates, coupled with the large error terms of regression models, suggests the likelihood that the pattern of downfalls is heavily influenced (although variably) by stochastic elements.

The Research Agenda

It should by now be clear that what separates our work from that of structural attributes researchers is a basic difference in the conception of the idea of stability as it is attached to cabinet governments. For us, cabinet stability is importantly dependent upon stochastic elements in the decisional environment (disruptive events), the timing of which are (largely) unpredictable in advance. Thus stability should be considered a continuous (rather than a discrete) quality of cabinets, where the continued existence of a cabinet is an interactive function of the impact of disruptive events (or events patterns) on decision making and some quality of resistance to such events that inter alia might be associated with structural attributes. Thus cabinets should be viewed as being more or less vulnerable to the continuous occurrence of randomly timed events over their respective tenures, with downfall representing, potentially, a transition from one state of dynamic equilibrium to another. Since we propose creation of a dynamic model of cabinet stability, we have been hopeful of discovering evidence supportive of an inference that the timing of cabinet dissolutions is influenced by stochastic elements. We feel that the evidence of our tests is sufficient to warrant further development of an events approach to the problem.[18]

While such a research agenda as we propose could be developed quite independently of a reliance upon game-theoretic representations of related governmental processes (such as government formation or the distribution of benefits), we wish to close by suggesting some implications an events approach to cabinet stability has for the development of coalition theory in general. As noted, we see the major contribution of an events approach to be the identification of a major source of uncertainty in the rational decision-making calculations of coalition actors. For if payoffs to actors are assessed in terms of benefits that are expected over some period of time in office, uncertainty regarding expected utility and the length of tenure might reasonably alter their decisions—both with respect to choice of partners and preferences for particular benefits—from what they would be if cabinet longevity were either irrelevant or fully predictable. Further, uncertainty as to longevity encourages us to view coalition processes dynamically, since actors may be expected to base their decision-making behavior on probability assessments about changing conditions (such as the reliability of a flow of benefits). This suggests that the articulation of game-theoretic explanations might profitably draw on recent developments that attend to metagames (supergames), relaxation of restrictive information assumptions, and reevaluation of the conflictual-cooperative nature of joint decision-making processes.

As is readily seen, our work on an events approach is in a very early stage of development and is not, as Strom has asserted, an attempt to displace the game-theoretic foundations of coalition research. The development of an events approach calls for a redirection of scholarly attention from the question of how long cabinets may be expected to endure toward the questions of when and why cabinets will fall. Our reconstruction of the central research question focusing on cabinet dissolution rather than duration accomplishes a great deal, for it alters the structure of the problem from one that seeks identification of new and improved structural attributes believed to promote (inhibit) intracoalition concensus to one that locates elements of stress and support encountered by decision makers over the period of their tenure in office. Strom seems to concede this point in the two paragraphs that precede his concluding section. We are encouraged that our work has apparently succeeded in promoting this reconceptualization of the problem.

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Notes

Strom wishes to thank John H. Aldrich, David Austen-Smith, Terence Ball, John R. Freeman, Robert W. Jackman, Brian L. Job, James D. Morrow, Bjørn E. Rasch, and W. Phillips Shively for valuable comments on previous drafts of this critique.

1. Cioffi-Revilla never addresses the theoretical foundations of the "deterministic" tradition. Its applicability to Italy is dismissed simply by the assertion that the causes of Italian government downfalls have been "numerous, varied, and essentially unpredictable" (1984, 334). This statement is followed by a selective catalog of Italian postwar cabinets, with a brief ad hoc description of the causes of their respective dissolutions. There is no semblance even of a

common conceptual framework that could permit analysis of these alleged causes.

2. Warwick, like Powell, reportedly is able to explain about half the variance in cabinet durability. However, Browne, Frendreis, and Gleiber (1986b, 632) fail to obtain equally impressive results in their attempt to replicate Warwick's analysis. The discrepancy appears to be caused by ambiguities in Warwick's case selection procedure.

3. Empirical studies often implicitly assume the legitimacy of treating individual governments as mutually independent events and of cross-national generalization. If such assumptions are not made, sample sizes may diminish substantially. However, even in single-nation studies, the number of cases is often comfortably large (e.g., 39 in Cioffi-Revilla's analysis).

4. Browne, Frendreis, and Gleiber implicitly recognize the problem of heterogeneity by estimating separate models for each country. However, this obviously still leaves them open to all the problems of intrasystem heterogeneity and contagion.

5. However, if the argument about payoffs as a function of time is taken to an extreme, it may eliminate the role of critical events in explaining government dissolutions. In other words, the payoff changes responsible for all government resignations could conceivably be accounted for by the passage of time (e.g., toward the next election).

6. The relationship between games and decisions is addressed in some detail by Luce and Raiffa (1957). They discuss the role of uncertainty in decision making and the theory of games. It should be clear that assigning the appropriate conceptual significance to uncertainty and adequately modeling it is critical to our formulation of the cabinet stability-duration problem.

7. Indeed there are voluminous literatures on coalition processes that have been developed in social psychology and sociology. For reviews, see Murnighan 1978 and Hinckley 1981.

8. Strom is critical of our failure (except by illustration) to imbue the theoretical element, "terminal events," with substantive content. Not only is his observation here correct but we consider this to be a major impediment to progress in theory development. Aware of this deficiency, we have begun investigating the problem and have reported some preliminary results of these efforts (Browne, Frendreis, and Gleiber 1986a; Browne and Gleiber 1986).

9. Although attributes researchers have never investigated the events environment of cabinets, an indirect assessment of the ability of structural attributes models to "predict" the events processes that result in cabinet dissolution inheres in the coefficient of alienation they produce. If we are correct in presuming that events (or events processes) are the proximate causes of cabinet downfall, then for structural attributes models to predict cabinet duration successfully, the appropriate regression model should specify events as intervening between attributes and duration. Since in existing structural attributes models no intervening events variables are specified, the implicit presumption is either that events are unrelated to duration or that their ability to bring down a cabinet (the timing of "terminal events") is completely determined by structural attributes. In either case, events are not seen to have any independent effect on duration. Thus, in structural attributes models, the coefficient of determination constitutes a measure of the degree to which events processes are statistically irrelevant to an explanation of cabinet duration.

10. Compare our enumeration of Scandinavian governments to Strom's (1986) appendixes.

11. Similarly, Lijphart's (1984) recommendation of a particular definition of durability is based in part upon the strength of the correlations between a set of systemic independent variables and the five alternative measures of durability he examines. However, contrary to Strom's assertion that Lijphart designates one definition as uniquely superior, Lijphart actually concluded, "All five measures are basically acceptable" (1984, 278).

12. Indeed, we have been quite explicit about this. See Browne 1982, 352–56; Browne and Franklin 1986, 479–80; Browne, Frendreis, and Gleiber 1984, 184–92; Browne, Frendreis, and Gleiber 1986b, 621; Frendreis, Gleiber, and Browne 1986, 621.

13. See Browne, Frendreis, and Gleiber 1986b, 636–40.

14. In our model, we use the theoretical concept "terminal event" as a hypothetical construct to provide the linkage between the observed distribution of interarrival times of dissolutions and the relatively large stochastic (error) terms in structural attributes models of cabinet duration. The "curve fitting" of which we are accused is the comparison of a derived expected distribution to an observed distribution. Regression models produce the same kind of comparison, but their basis is the inductive estimation of a BLUE curve to represent the theoretical, or expected, distribution. This leads us to conclude that our work is more squarely in the deductive tradition Strom has been advocating. Furthermore, empirical tests of game-theoretic models (preferred by Strom) depend on the same sort of comparison of expectation and observation as ours do. The resulting analysis is an uncontrolled (no inclusion of the structural attributes explanation) model of what we believe to be the primary component of an explanation of government dissolution.

15. This is where Strom is confused by our use of the term *alternative hypothesis*, by which we mean research hypothesis. The usual statistical test is to reject the null hypothesis while contingently accepting an alternative, the empirical estimate, as a research hypothesis. By implication, failure to reject the null hypothesis warrants rejection of the alternative

research hypothesis.

16. We are unfamiliar with any work supporting Strom's assertion that fewer degrees of freedom reduce the power of the chi-square test. We assume Strom is actually referring to small expected cell frequencies, a point we specifically addressed (Browne, Frendreis, and Gleiber 1986b, 642, n. 11). For additional consideration of this question, see Chapman and Schauffle 1970, especially chapter 11. Strom also makes inappropriate use of the technical term *power* (1-beta) which refers to the probability of rejecting the null hypothesis. Although *power* is related to confidence, sample size, and effect size, *power* is not an issue in our research, because we do fail to reject the null hypothesis. The inconclusiveness of our empirical test across countries may result from the contamination of the simple univariate model by factors other than events (especially those determinants of duration identified by the traditional literature) that are uncontrolled in our test. Despite this limitation we obtain strong positive empirical results.

17. While our empirical work is not derived from a fully articulated theory, the earlier structural attributes work is equally vulnerable to such criticism. This work, for example, typically employs statistical models that are not the derived implication of any theory; the properties of the regression models employed in this research (e.g., linearity) have not been justified, either verbally or mathematically, in discussions of the hypothesized relationship between attributes and duration.

18. Our basic conception of stability anticipates the potential fruitfulness of nonlinear dynamic modeling. Relevant literature supportive of this view is rapidly developing in mathematics, the physical and biological sciences, and information theory. For a reasonably comprehensive survey of these developments, see Gleick 1987 and Campbell 1982. Unsurprisingly, this methodology has been slow to be disseminated into the social sciences, but relevant applications have been undertaken in economics with reference to the long-term fluctuation-stability of prices in both the cotton market and the stock market and in political science in an examination of regime stability in pre- and postrevolutionary Nicaragua (James 1988). Additionally, dynamic modeling of international conflict processes and alliance behavior has recently gained a secure position in the international relations literature. Here, prominent examples include work by Michael D. Ward (1982a, 1982b), Manus Midlarsky (1981), Robert T. Holt, B. L. Job, and L. Markus (1978), and several selections in a compendium on breakdown in the international system, edited by Dina Zinnes (1983).

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IS OPPOSITION TO NUCLEAR ENERGY AN IDEOLOGICAL CRITIQUE?

In the June 1987 issue of this Review, *Stanley Rothman and S. Robert Lichter offered evidence to support their argument that "the new environmental movement in the United States is partly a symbolic issue," that elites in the news media and in public interest groups misrepresent the dangers of nuclear energy as a surrogate for more direct criticism of liberal capitalism in the United States. In this controversy, Charles J. Helm expresses skepticism about the Rothman-Lichter line of argument; and they respond.*

Rothman and Lichter (1987) argue that within the last 20 years there has been a significant shift in public attitudes over the risks attendant with nuclear energy. Drawing on a content analysis of the media's coverage of nuclear energy and a survey of attitudes in the media elite, they find evidence of biased stories authored by an elite skeptical of nuclear energy and critical of U.S. society. They conclude that the media elite gravitated to environmental issues (consciously or not) as a "surrogate" for an underlying ideological critique of U.S. society that was necessitated by the demise of movement politics in the mid-seventies and the absence of a leftist political party as a more traditional channel of political critique. The main thrust of my remarks will center on two points central to their argument: first, the posited link between public attitudes and a biased media elite and second, the Left's search for a "surrogate" avenue—environmental issues—when other outlets were foreclosed.

Causal Link

The time sequence for the posited link between media bias and public skepticism over nuclear energy, as in any causal argument, is controlling. Is there any evidence, in Rothman and Lichter's account, that media coverage of nuclear energy underwent a shift to a more critical mode prior to the public's growing doubt over nuclear safety? They seem to feel that if media and public attitudes have changed on the issue of nuclear safety then the argument for causal sequence is implicit in the relation between the media as catalyst and the public as receptacle. As evidence of the causal sequence they offer the argument that since media coverage and public attitudes in the fifties and early sixties were sympathetic to nuclear energy and now a "substantial majority" is opposed to any new nuclear plants, the major source of that change is, most likely, fears generated by the media during the seventies. (The confidence of the scientific community on nuclear energy has remained constant throughout.) The unstated assumption here is that the public is poorly informed and heavily dependent on media cues for any shift in public attitudes. At some point in the late sixties, according to Rothman and Lichter, the media apparently grew increasingly out of sync with public attitudes on safety in nuclear plants. But for all the care and sophistication they have shown in the development of their surveys and supporting analysis, they offer no evidence and little argument to support any shift in media coverage preceding a shift in public attitudes. Even if the media consensus now is not in accord with the present scientific consensus on the safety of nuclear energy, that is no evidence of the time sequence *or* of a causal link.

While we lack any direct evidence—surveys of attitudes in the media elite or content analysis of the media's coverage of nuclear energy during the sixties—of the transition in media coverage, there is indirect evidence that suggests the need for caution in trying to isolate the causes of a shift in public attitudes. Are the media, as Rothman and Lichter imply, the major arbiters of public attitudes? The recent controversy surrounding the disease AIDS might prove instructive here. The scientific community has almost uniformly contended that AIDS cannot be spread through casual contact. More specifically, unless one either is sharing needles or engaged in intimate sexual relations with a suspected AIDS carrier or is the unfortunate victim of a tainted blood sample, the danger is minimal. The media, in contrast to its account of the risks attendant on nuclear energy, has generally been quite "responsible" in reporting the scientific consensus on the medium of AIDS exchange. Apparently these careful admonitions have fallen on deaf ears. In some school districts, AIDS victims have been forced to withdraw from the public schools; and in California a referendum that would have required the quarantining of suspected AIDS carriers, while handily defeated, still drew considerable support. What is the source of such hysteria, fear, and prejudice? Why isn't the public listening to its "leaders" as reported by the media?

The obvious rejoinder here is that nuclear energy and AIDS really are not comparable. AIDS is a disease that has only received public scrutiny for the past few years and is confined in large measure to a population outside the mainstream. There is no cure in sight and the relentless figures on deaths from the disease are rising at a geometrical rate. Despite the accidents at Chernobyl and Three Mile Island, there have been far fewer deaths directly tied to nuclear energy. Nuclear energy has been on the public agenda since World War II and the links between illness or deaths and nuclear energy are considerably more tenuous. These contrasts, though well taken, seem to me insensitive to public concern, mistaken or no, on issues like nuclear energy and a disease like AIDS. Well before the supposed shift in media coverage, public fascination and fear of the atom was much in evidence. During the fifties there were countless "mutant" movies (Boyer 1985; Oakley 1986) clearly tapping public fears and suggesting the untoward effects of radiation exposure on humans and animals (ants, tarantulas, bees). The atom has been a powerful metaphor suggestive of both life and death, beauty and destruction, the beginning and the end. The public has not been willing to allow the scientific community the last word in shaping their perceptions of this mysterious and awe-inspiring phenomenon. In the work of Lippman (1922), Edelman (1967), Bennett (1987), and Nimmo and Savage (1976) on images, symbols, and stereotypes, we have some sense of the possible disjunction between image and reality and the types of issues that are more prone to these sorts of distortions. The public hysteria (within some communities) over AIDS or the atom or both and the disparity between elite and mass attitudes should offer a cautionary tale for those tracing lines of influence between the media and its audience.

Surrogate Thesis

Rothman and Lichter have rather nicely tapped a disjunction between the scientists' confidence in nuclear energy and public skepticism. They are sufficiently surprised and disturbed by their findings to feel that it warrants talk of a leftist surrogate manipulating (consciously or no) public attitudes through the media. But perhaps the explanation they seek is far less oblique: less psychological and more materialist. Lipset and Schneider, in *The Confidence Gap* (1987), after reviewing hundreds of public opinion surveys over the past 20 years, have shown a steady decline in public confidence in institutions at all levels in U.S. society. While they second the Rothman and Lichter account that the press has tended to give a rather bleak picture of business and government on environmental issues, they reject "the assumption that this emphasis is largely a reflection of a liberal bias" in the media, suggesting instead that it is "the desire of journalists to find exposés, to get the story behind the official report. There is a story in reporting the 'failure of . . . institutions'; the version that reports successes is viewed as public relations" (Lipset and Schneider 1987, 438). As to the source of this loss of public confidence in our institutions, rather than a leftist media in search of a surrogate avenue, they suggest a more mundane answer: "Vietnam, Watergate, the energy crisis, recession and hyper-inflation demonstrated . . . that our institutions generally, and the federal government in particular, had failed to perform" (p. 436).

The evidence for the "surrogate" thesis is of course indirect. Much of the force of that thesis rests on a contrast Rothman and Lichter draw between the Left in France and in the United States. In France, "the political left does not require surrogate issues. It can still mount a direct attack upon the existing social and political order" (p. 397). Lacking the need for surrogates, "the French Communist party is strongly supportive of nuclear energy, and the French Socialist party at least moderately so" (p. 402). The absence of an institutionalized Left in the United States has apparently created both an alternate outlet (the environmental movement) and a pseudo-need (concern about nuclear safety). The French Left, less frustrated than their U.S. counterpart, have no need for surrogates and remain largely indifferent to environmental issues. (I take it that it would follow from their argument that if—apparently a very big *if*—the French Left ever makes a "frontal" attack on environmental issues they will see them "clearly," that is, bereft of any ideological tainting). Environmental beliefs in the United States are perhaps "sincerely held" on Rothman and Lichter's reading, but they are also evidently the dyspeptic substitute for the failures of leftist politics. Whether purposely or not, one effect of their argument is to delegitimize claims of the environmental movement—both within the media and without—with respect to safety and nuclear energy. They have moved from a critique of the environmentalists' positions to the structural dislocations—both institutional and psychological—of leftist politics.

In an article so "sensitive" to the Left's ideological tainting of the issue of nuclear safety, it is disquieting to see a comparative scholar of Stanley Rothman's stature select France as a relevant counterpoint to U.S. environmental politics. As they note, only to dismiss it just as quickly, France is something of an aberration in Europe, combining an aggressive nuclear energy program and a weak environmental movement. West Germany—or Sweden or Belgium—would provide a more challenging counterfactual test of their "surrogate thesis" and the posited link between the media, nuclear energy, and the Left. Presumably, West Germany would also fit their model of a political system with an organized Left—the Social Democratic Party (SPD). Even though the SPD was the party in power during much of the early history of the "citizen initiative" movement in the late sixties and early seventies, these groups began outside the traditional channels of neo-corporatist politics in West Germany (Conradt 1982). It was their sense of the *closure* of German politics, even within the organized Left, that necessitated the need for *alternativen.* The Greens only organized nationally in 1979 to consolidate a massive citizen protest against any further building of nuclear energy plants and only entered the Bundestag as a recognized party in 1983. While the SPD and the Greens have formed coalitions in some *länder,* there is still a quite tenuous truce between the traditional Left and the Greens. (During party conferences the Fundies—a radical wing of the Greens—have repeatedly voted to withdraw from parliamentary politics.) Why did one segment of the West German Left, with the collusion of some traditional agrarian conservatives like Herbert Gruhl, attach themselves to a surrogate environmental movement even while the SPD—though weakened—remained quite powerful? What is the source of this frustration and denial of traditional leftist politics in West Germany? Have scientists in the Federal Republic reached a more critical consensus on nuclear safety?

The plot thickens even further if we bring in the other missing variable—the West German news media. In the United States, Rothman and Lichter argue, media bias has been, at least in part, responsible for the public shift in attitudes. As any introductory text on German politics makes clear (Conradt 1982; Edinger 1977; Rothman 1970), German television and radio is administered by state-based independent boards. While the networks possess considerable autonomy, the establishment political parties make sure their voice is heard on these boards. Within both German television and the independent press, there is also no developed tradition of muckraking or investigative journalism. (There are several exceptions, the two most obvious being the weekly magazines *Stern* and *Der Spiegel.*) The Axel Springer conglomerate controls a large part of the German press (its papers draw over 60% of the readership) and is staunchly antileftist with the Greens a favorite target (Conradt 1982). Many newspapers are also frequently identified with a particular party and tend to follow party policy quite closely. (During much of the seventies, the SPD and Christian Democratic Union [CDU] *party* line was quite supportive of a nuclear energy program.) Whatever the evidence for media bias toward the nuclear industry in the United States, the picture is considerably more mixed in West Germany. Why—given the absence of systematic media bias and the presence of an organized Left—has the antinuclear movement (critical of both nuclear weapons and plants) been so strong in West Germany amongst such a wide segment of the German population?

The source of public attitudes on issues like nuclear energy are really quite difficult to trace; and for all the care that Rothman and Lichter have shown in their surveys and analysis, their linkage of media bias and public attitudes is still not proven. Their suggestion that the environmental movement has served as a "surrogate" for an underlying ideological critique is not supported by the West German example. And finally, the "surrogate thesis" implies both that the Left was forced to seek out alternate avenues of expression and that they found their voice and target as a result of the failures of leftist politics. Such a displacement thesis demands quite compelling evidence, particularly since less circumspect explanations are readily available (e.g., Lipset and Schneider 1987).

CHARLES J. HELM

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Opposition to Nuclear Energy

Charles Helm raises two issues: (1) he denies that we have demonstrated either that the U.S. national media have contributed substantially to nuclear energy's fall from grace in this country or that ideology has influenced their coverage; (2) he questions whether we have demonstrated that opposition to nuclear energy is partly a surrogate for more direct attacks on the political system. He rejects our comparison with France and suggests that if we had turned to other countries, our argument would be less persuasive. After all, a party of the Left (the Social Democrats) exists in Germany, and the Left has mounted an attack on nuclear energy.

Before we turn to the main body of our reply, we should make two points. First, we agree that nuclear energy has several strikes against it and that public anxieties about it are easily aroused (see p. 401 of our article). We explicitly state that such public concerns about nuclear energy and other environmental issues are not without foundation. In dealing with one factor that has played a role in the ongoing public debate, we do not deny the relevance of other concerns. Our aim is not to discredit antinuclear activists by examining their motives but rather to understand the process of social change.[1]

Second, we do not believe that we have proven our hypotheses in any absolute sense, only that we have established a prima facie case. Most of the issues we raise require a far more in-depth treatment than we could provide in the article. We plan to do so in Rothman, Lichter, and Lichter 1988. Even then, our evidence will necessarily lack the compelling quality it is possible to achieve in some areas of the natural sciences.

Let us turn first to the issue of media influence. We do not maintain, as Helm suggests we do, that the disjunction between public and scientists' views establishes that the media are responsible for changing public attitudes. Our argument is more complex. Specifically we demonstrate that (1) scientists overwhelmingly support the development of nuclear energy and believe that nuclear plants are safe;[2] (2) media reporting suggests that the scientific community is sharply divided over nuclear safety, with perhaps a majority opposing the development of nuclear energy; (3) elite and public perceptions of the scientific community's views more closely resemble media reports than reality (as measured by our surveys of scientists); (4) elite views of nuclear safety (a key determinant of policy preferences) correlate with the perspectives presented on media outlets that they consider reliable. People trusting antinuclear news outlets are less likely to believe that nuclear plants are safe than are those persons who trust pronuclear media outlets. So we do not *assume*, as Helm argues we do, that the public is badly informed. We demonstrate that both the public and various leadership groups are misinformed and that their misinformation seems to be related to media coverage of the issues involved.

It is also logically possible that prior ideological convictions drive *both* nuclear attitudes and media consumption. Therefore we agree with Helm that our case would be strengthened by demonstrating that changes in media coverage of nuclear energy *preceded* changes in public views of nuclear energy. In fact, we have developed this evidence on television reporting and public attitudes for the 1970s. As Table 1 demonstrates, the decline in positive public attitudes toward nuclear energy was preceded by both heavier and increasingly negative television coverage, even before the accident at Three Mile Island. Note also that as coverage grew more negative, it also increased in absolute terms and became more focused on reactor safety.

Helm attempts to demonstrate that the media's effect on public opinion is not very great by citing media coverage of

Table 1. TV News Coverage and Public Opinion on Nuclear Safety

Time Period[a]	Stories per Year[b]	Reactor Safety Stories (%)[c]	TV Slant (%)[d]	Public Opinion (%)[e]
1971				32 (57 – 25)
1970–75	26	8	-14 (25 – 39)	
1975				23 (55 – 32)
1976–78	50	17	-24 (12 – 36)	
1978				2 (43 – 41)
1979	518	41	-30 (17 – 47)	
1980				-35 (28 – 63)

[a]Some of the data is time-discrete and some is time-aggregate. Figures were unavailable for 1969.

[b]Average yearly number of nuclear energy stories.

[c]Percentage of all nuclear-related stories dealing with safety of nuclear reactor operation. (*Source:* Lichter, Rothman, and Lichter 1986, 185–86, 203, and recalculation of our data.)

[d]Percentage of positive stories minus percentage of negative stories. (*Source:* Lichter, Rothman, and Lichter 1986, 204 and recalculation of some of our data.)

[e]Percentage in favor minus percentage opposed. (*Source:* national Harris and Gallup surveys cited in Rankin et al. 1981, 30–31.

AIDS. He asserts that while such coverage has been very responsible, the public has still responded in a highly panic-stricken manner. Although systematic content analysis is lacking,[3] nevertheless it is our impression that Helm is probably correct in his assessment of media coverage. We disagree, however, with his characterization of public reaction: despite his anecdotal evidence, the panic has thus far been relatively minimal *(New York Times,* 17 February 1988).

The key comparison is this: imagine how the public would react if the media featured stories that gave the impression that a significant portion of the scientific community believes AIDS *can* be contracted through casual contact with infected individuals. Recall the recent wave of media-borne criticisms that descended upon Masters and Johnson for suggesting this and on *Newsweek* for publicizing their study.

We never suggested that the media are all-powerful in shaping public views. Our argument is merely that they play an important role in the formation of such opinion. Helm cites Lipset and Schneider's *The Confidence Gap* (1977) to demonstrate that journalists' reportage is shaped by the desire to expose (rather than by political views) and that the media have little influence on public opinion. We never argue that journalistic coverage is determined entirely by the political views of journalists, only that their views do influence coverage. We offer considerable evidence for that proposition in our article, as well as our book, *The Media Elite* (Lichter, Rothman, and Lichter 1986).

Further, Lipset and Schneider's own evidence does not support the assertion quoted by Helm. For one thing, they do not examine journalists' attitudes or coverage of issues. Their bad news hypotheses is unable to explain media coverage of AIDS, nor can it explain positive media coverage of busing, as we document in *The Media Elite.* Second, as Lipset and Schneider point out, the public's trust in business in the abstract was declining at the very same time that their personal experience with business continued to receive high ratings. In addition, Lipset and Schneider found an inverse correlation between trust in government and trust in the media. Finally, Democrats and liberals trust the press more than do Republicans and conservatives (Lipset and Schneider 1987, 99, 122–25). It is unlikely that these correlations are accidental.[4]

Helm implies that U.S. citizens came to reject nuclear energy because they did not trust scientists sufficiently to give them the last word on the issue. He may be right. We shall never know because, as our article demonstrates, the public in the early 1980s still trusted "independent" scientists' views on nuclear energy but were misinformed about such views (p. 384).

The surrogate issue, Helm's second concern, is a very complex one, which we plan to develop in more detail in our forthcoming volume, *Elites in Conflict* (Rothman, Lichter, and Lichter 1988). We will limit ourselves here primarily (and briefly) to Helm's criticism of our choice of France as the country with which to compare the United States and his use of Germany as a counterexample. We do have some data on the issues that he discusses. We should, however, note that one characteristic of all European countries is the considerable influence of the U.S. media. The United States' declining economic power has yet to reduce its cultural hegemony (Richard Grenier, "Around the World in American Ways," *Public Opinion* Feb./Mar. 1986, pp. 3–5, 58–60; Jack Valenti, "And the Winner Is . . . American Movies, Television, and Videos," *Public Opinion* Feb./Mar. 1986, pp. 13–15). There is little question but that the U.S. media helped bring the student movement to full flood in Europe in the 1960s (Rothman and Lichter 1982), just as they helped bring the nuclear energy issue to a world wide public. Further, as our article points out, a perspective, once institutionalized, may draw upon many other motives in addition to that which fueled the initial movement. One must be careful not to extrapolate backward from the motives and arguments used in a movement at full flood (or in decline) to its sources.

Having said all this, the German case better supports our argument than Helm's. First, radicalism in West Germany has lacked a political outlet since World War II, although for somewhat different reasons than in the United States. The Communist party was effectively destroyed by the war and the creation of East Germany; the SPD jettisoned its Marxist program in 1959 and joined the Christian Democrats in a coalition government in 1966. The emergence of the student movement of the late 1960s was partly a function of the lack of an outlet in the parties for feelings of radical alienation from the system. Helm makes this point himself.

Despite its slight shift to the Left during the 1970s, the SPD remains, in the eyes of alienated students and other intellectuals, a bastion of the system that lacks a genuine commitment to radical change. We submit that the Greens provided an outlet for themes of alienation that could not be expressed through the SPD or other conventional structures. The growth of the Greens has pushed the SPD to the Left, but it remains a party of the establishment.

Helm's position on the German media's treatment of nuclear energy is refuted by systematic survey and content analysis data.[5] First, as we point out in our essay

(p. 387), the German scientific community is as supportive of nuclear energy as is its U.S. counterpart. The study we cited there (Institut für Demoskopie 1985) also found that journalists were skeptical about nuclear safety and that the public was not aware of the extent of the support of the scientific community for it. In short, their findings replicate ours.

More recently, H. M. Kepplinger and Rainer Mathes of the Institut für Publizistik at the University of Mainz studied nuclear energy coverage in seven major West German print outlets over two decades (1965–86) and found the same rise in negative coverage that we did. They also studied coverage of several other environmental issues such as air and water pollution. They conclude, "When we look at reality, we can ascertain that the environmental situation has improved, but when we look at the media's version of reality, the environmental situation continues to deteriorate." They analyze this coverage in terms of a new ideological perspective which "took hold of journalism in the mid-1970s." As they note, "it was not the reality which changed, but the perspective. This new portrayal of reality by the media led to a fundamental change in the public's views" (Kepplinger and Mathes 1987, 15-16).[6]

Any attempt to explain attitudes toward particular issues as unconscious surrogates for attitudes toward other issues is impossible to establish definitively, certainly in a short essay. This, of course, is true of most propositions of any interest in the social sciences. Nor do we claim that the surrogate argument fully explains what has been happening in Germany and the United States. Real issues are involved and massive shifts are taking place in European and U.S. social and political life. We hope to deal with many of these in *Elites in Conflict.*[7]

Nevertheless, environmentalism was and is a surrogate issue for many on the Left. Our correlational data support this argument, as do the statements by such people as Dellinger and others quoted in our essay.[8] As the environmentalist scholar Langdon Winner points out in *The Whale and the Reactor* (1986, 142),

> In a society strongly committed to capitalism as a way of life it is difficult to address the social ills of capitalist practices head on. A more subtle tack is to begin discussing urgent issues that do not appear to have any ideological valence at all. In pointing to this strategy I do not wish to dismiss any of the claims made by these political activists or writers. But it is fair to say that this approach . . . is another example of a conceptual Trojan horse. If it were effective to tackle social injustice and the concentration of economic power directly, I suppose we would do it more often.

Anyone familiar with Winner's work knows that he does not write as he does to undermine environmentalist arguments. Neither do we, necessarily, although we are certainly less sympathetic to certain aspects of the new environmental movement than he is.

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Notes

1. We do not say that environmental beliefs are "perhaps" sincerely held. We say that they are sincerely held. Here, as elsewhere, Helm does not accurately describe our argument.

2. They still do, even after Chernobyl (*Media Monitor* [Washington: Center for Media and Public Affairs] May 1987, pp. 1–2).

3. The Lichters' Center for Media and Public Affairs is presently conducting an in-depth analysis of the issue.

4. For an extensive survey of the literature on the influence of the media, including the Lipset and Schneider 1987 study, see Lerner and Rothman n.d. We do not characterize journalists as being on the "Left." We do offer evidence that they are liberal and more likely to trust radical scientists than to trust the scientific establishment.

5. Helm erroneously cites Rothman's earlier work (1970) to support his case, which it does not. (See Rothman, Scarrow, and Schain 1976, 121; see also Noelle-Neumann 1984.)

6. The outlets studied were *Welt, Frankfurter, Allgemeine Zeitung, Suddeutsehe Zeitung, Frankfurter Rundschau, Spiegel,* and *Stern.*

7. See also Rothman 1984.

8. Helm quotes us as saying that the nuclear energy issue became a surrogate for the failures of left-wing politics. Our point is rather that given the hegemony of liberal capitalism in the United States, there has never been a successful radical movement. The radical movement didn't fail, it never got off the ground.

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Forthcoming in December

The following articles and research notes have been tentatively scheduled for publication in the December 1988 issue:

Charles J. Barrilleaux and Mark E. Miller. The Political Economy of State Medicaid Policy.

Courtney Brown. Mass Dynamics of U.S. Presidential Competitions, 1928–1936.

Gregory A. Caldeira and John R. Wright. Organized Interests and Agenda Setting in the U.S. Supreme Court.

John S. Dryzek and Stephen T. Leonard. History and Discipline in Political Science.

William A. Galston. Liberal Virtues.

Ronald Inglehart. The Renaissance of Political Culture.

Mark S. Kamlet, David C. Mowery, and Tsai-Tsu Su. Upsetting National Priorities: The Reagan Administration's

Budgetary Strategy. A Research Note.

Patrick J. Kenny and Tom W. Rice. Presidential Prenomination Preferences and Candidate Evaluations. A Research Note.

Robert C. Luskin and Carol Cassel. Simple Explanations of Turnout Decline. A Research Note.

Terry Moe and John E. Chubb. Politics, Markets, and the Organization of Schools.

John D. Robertson and Marick F. Masters. Class Compromise in Industrial Democracies.

Lawrence S. Rothenberg. Organizational Maintenance and the Retention Decision.

Arlene W. Saxonhouse. The Tyranny in the World of the Polis.

H. P. Young. Condorcet's Theory of Voting.

REVIEW ESSAY

SCHOLARSHIP ON PRESIDENTIAL SELECTION IN THE UNITED STATES

BYRON E. SHAFER
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Media and Momentum: The New Hampshire Primary and Nomination Politics. Edited by Gary R. Orren and Nelson W. Polsby (Chatham, NJ: Chatham House, 1987. 200p. $20.00, cloth; $11.95, paper).

A Horse of a Different Color: Television's Treatment of Jesse Jackson's 1984 Presidential Campaign. By C. Anthony Broh (Washington, DC: Joint Center for Political Studies, 1987. xiv, 93p. $7.95, paper).

Selecting the President: The Nominating Process in Transition. By Howard L. Reiter (Philadelphia: University of Pennsylvania Press, 1985. xviii, 192p. $22.50, cloth; $8.95, paper).

Presidential Selection. Edited by Alexander Heard and Michael Nelson (Durham, NC: Duke University Press, 1987. x, 413p. $50.00, cloth; $17.95, paper).

News That Matters: Television and American Opinion. By Shanto Iyengar and Donald R. Kinder (Chicago: University of Chicago Press, 1987. vii, 187p. $22.50).

The arrival of another presidential year is an opportunity to investigate the progress of scholarship on presidential selection. It is also a harbinger, whatever that verdict, of a spate of additional works on the topic. The basic phenomenon that guarantees both this opportunity for reconsideration and this promise of additional scholarly work, however, is not so much the recurring event itself, the politics of presidential nomination and election. Rather, it is the fundamental and apparently reliable *unpredictability* of that politics. This unpredictability is, of course, an implicit warning to anyone undertaking such a review: what we think we know may shift with alarming speed here. Nevertheless, such a review still inherently represents a nested set of opportunities. It is an introduction, first and foremost, to a group of individual books. It is a chance, through them, to examine the general state of knowledge in the field. It is, inherently and simultaneously, an opportunity to examine the way in which a scholarly community has gone about the task of developing that field. It is, as a result and finally, an invitation to look at the degree to which practical politics, scholarly work about it, and the overall organization of that scholarly work have come to cohere in a reasonable whole.

The normal scholarly urge toward systematic understanding—in this case, the search for recurring factors that shape the politics of presidential selection—has in fact characterized most of the work in this burgeoning field, as it has certainly characterized each of the scholarly works that are the specific impetus to this review. Yet it is an even more pervasive sense of unpredictability, of a new political world that is both different and *recurrently different,* that ultimately impels this particular scholarly quest. The usual dividing line for this new and insufficiently explained politics is the period between 1968 and 1972, as the origin of a set of extensive formal changes in the procedures of presidential nomination, which have ramified (and ricocheted) across the ensuing years. Yet many scholars have come to see these reforms as themselves the product, and then the embodiment, of other large changes in the society around them, changes that might not have featured as precise a breaking point but which certainly promised a very different sort of presidential politics, in and of themselves.

In any case, when a rapidly changing society meets a reformed institutional structure, initially unpredictable outcomes are almost guaranteed. And unpredictable outcomes there certainly were: the McGovern nomination, the Nixon landslide and its unhappy aftermath; the Ford presidency, the Carter nomination, and its surprising result; the Kennedy challenge, the Reagan nomination, the Reagan presidency and its alleged revolution—these are only the most stark summary events in a chain of major and minor surprises. More to the point, this tendency toward what might be called "microsurprise" has not, to all appearances, abated. As this is written, presidential 1988 shows every indication of taking an equal place on the chain, suggesting—perhaps superficially?—that the institutionalization of unanticipated consequences is the defining characteristic of the new era in presidential politics. At this peculiar level of analysis, then—the level involving the prediction of precise twists and turns within a complex, diverse, and amorphous national process—political scientists have probably been no more prescient than journalistic commentators or the intelligent lay public.

Where they have had a comparative advantage, instead, has been in two other realms. The first is in possessing an intellectual tradition that reduces the onus of an inability to preduct precise (sub)outcomes. From this perspective, a composite social system with numerous influences on a given result—indeed, numerous partially independent influences, which vary both in relevance and in strength—*should* put inherent limits on the specificity of prediction. Such a system can still be analyzed and explained satisfactorily, through its composite elements—while defying, in principle, precise prediction. The second advantage is in belonging to a larger professional community that could go to work on the diverse elements of apparent anomaly in a new presidential politics and attempt to make those elements individually comprehensible in a systematic fashion. The five works at the center of this review essay are all a product of that orientation, by that professional community. Each in fact proceeds by isolating particular aspects of the politics of presidential selection, trying to treat them systematically, and trying to use that treatment to gain insights on the larger whole.

In *Media and Momentum: The New Hampshire Primary and Nomination Politics*, Gary R. Orren and Nelson W. Polsby have collected essays organized around a particular and strategic contest site, and have used that site to integrate diverse observations about the politics of presidential selection. In *A Horse of a Different Color: Television's Treatment of Jesse Jackson's 1984 Presidential Campaign*, C. Anthony Broh has focused instead on a particular candidate as he appeared in network television news and has used the distinctiveness of the resulting portrait to comment on various facets of the process. In *Selecting the President: The Nominating Process in Transition*, Howard L. Reiter has sought instead to survey major elements of the nominating process, both formal and informal, so as to get at an array of central products. And in *Presidential Selection*, Alexander Heard and Michael Nelson have tried to isolate various subaspects of nominating politics, assign each to individual authors, and reassemble the product, side by side, in a single volume. How have they, collectively, done?

In formal terms, U.S. presidential nominations are accomplished through a succession of state primary elections and state party caucuses, in which the outcome of preceding contests is likely to have major impact on those which follow. *Media and Momentum,* conceived, assembled, and edited by Gary Orren and Nelson Polsby, focuses on a key point, the New Hampshire primary—disproportionately *the* key point, as many of the authors note—to illuminate the larger dynamic and its products. Data on demographic and political change in New Hampshire, in three of their chapters, emphasize the importance of *sequence* by showing its continuing influence under changed social conditions. Extended analyses of press coverage, at the center of three other chapters, explain the *means* by which a key early placement in this sequence attains its influence. The longest and most ambitious chapter then moves on from New Hampshire to the process as a whole. The success of this volume in using New Hampshire as a "window" on the larger politics of presidential selection is such that it may well spawn successors—perhaps on the Iowa caucuses and nominating politics, the southern regional primary and nominating politics, and so forth. If these meet the same standard, they may rightfully be welcomed.

In informal terms, U.S. presidential nominations are also powerfully shaped by the available field of candidates for the total contest and not just by its sequence of its events. Different candidates mix different constituencies and issues—not to mention different skills—in different ways; nothing can guarantee a particular sort of field in any given year. *A Horse of a Different Color,* from Anthony Broh, focuses on one of these candidates, Jesse Jackson, in 1984, though the analysis of press coverage presented in this short monograph features continued comparisons with the other major candidates in the Democratic field of that year as well. Broh finds press treatment of Jackson to have been relatively evenhanded and easily explained, though rather consistently undercutting his nominating potential, a perspective contrasting—on the order of the bottle being half empty versus half full—with the long essay from Henry E. Brady and Richard Johnston in *Media and Momentum,* which finds Jackson to have been overly covered when judged by horse race standards and overly well treated by comparison with the other candidates. An evident sympathy for the candidate and his larger cause, however, in no way mars the extensive tables in *A Horse of a Different Color,* which can surely be used to address the fate of other individual candidates or of a composite field.

A very useful effort to marry the formal and informal influences on presidential selection is Howard L. Reiter, *Selecting the President.* Reiter was among the first—and remains among the most determinedly empirical—to argue that recent changes in the character of nominating politics have as much or more to do with larger social changes as with procedural reforms. In this, the decline of political parties carries a heavy portion of his argument, but *Selecting the President* goes on to search for further elements of continuity (and change) in presidential nominations after that decline. The pursuit of this goal is occasionally overzealous. One need not, for example, strive to fit all of presidential politics into one among two or three integrating hypotheses: the outcome of nominating politics may well be best explained by evolutionary forces, the character of convention delegates by procedural reform. Just as one need not insist on methods in the narrow, to the point where they obscure larger explanations: rigor *can* become rigidity, and all tables are not created equal. Yet the larger continuing argument about the roots of the current process in evolutionary social developments is almost surely right—and convincingly sustained—while there is an absolute goldmine of research and information, carefully extracted and clearly presented, in the material mobilized to support that argument. This material seems destined to be integrated quickly into our general understanding of nominating politics.

A stimulating supplement to most such work to date is the collection in *Presidential Selection*, commissioned by Alexander Heard and edited by Michael Nelson. The working papers for a study funded by the Sloan Foundation on the nominating process, these chapters have the distinct advantage of *slighting* the conventional aspects of presidential selection—rules for delegate selection, dynamics of nominating campaigns, and even patterns of media coverage—in order to concentrate on other, consequential, less frequently considered topics. Most authors explicitly share the basic premise of Reiter in *Selecting the President*, that the weakness of party-as-organization is central to their particular focus; though they would differ tremendously (with him and among themselves) over the virtues of the result. The most provocative of these essays of almost uniformly high quality are probably those on implications for international policy and comparative policy-making, from Ralf Dahrendorf, Ernest May, and Richard Rose. These add an aspect of the debate over U.S. selection procedures that is often completely absent, though they also proceed with their own somewhat ironic avoidance of the fact that European selection processes appear increasingly to follow "the U.S. model." Not just these essays but almost all those in the volume, however, are additionally distinguished by their willingness to address available structural options to current practice, with a clear and simultaneous realization of the limits of the possible in such restructuring.

Each of these works explores, carefully and in detail, one or more major elements shaping the politics of presidential selection. Each also attempts to use its particular approach to gain some additional purchase on the larger process. Accordingly, each implicitly proceeds on the basis of two further, larger, linked assumptions. The first is that a focus on the full and careful exploration of major *elements* within this politics is the best way to move toward a more comprehensive understanding. The second (and more directly methodological) is that the actual operation of the politics in question is subject to so many lesser but intermittently consequential influences and that these influences are so partially and loosely connected to each other as to make this the most appropriate way to proceed. While each individual author might prefer his own summary statement, then, each proceeds *as if* he were part of a larger scholarly community, the sum of whose investigations will in fact explain the phenomenon in question, just as each proceeds *as if* that phenomenon, even more fundamentally, was itself structured so as to require this approach.

If these individual authors had reason to stop and address this second and more basic assertion—that the world of presidential politics is subject to numerous, intermittently influential, only partially connected factors and that this justifies the evolved way of addressing that world—they would find that they already possess substantial theoretical arguments, from related realms, which could be mobilized to support it. Or at least the standard arguments about the distinctive characteristics of U.S. politics would serve quite well to defend this way of proceeding—indeed, to explain why it is the essential way. That received wisdom, at its most global, asserts, first, that U.S. society is characterized by multiple, overlapping, but shallow social cleavages; second, that the politics flowing from that society is characterized by weak intermediaries, and hence by segmented and evanescent coalitions; and third, that the institutions driven by these intermediaries are themselves divided, competitive, and facilitative of perpetual (re)negotiation.

In other words, an additional virtue of these individual works—from Orren and Polsby, Broh, Reiter, and Heard and Nelson—is to provide detailed instances of these larger generalizations about U.S. politics, instances characterizing the more specific politics of presidential selection. Together, for example, these books certainly convey the sense of a politics built on numerous and overlapping, but not terribly deep, social cleavages. This is, after all, why the *sequence* of individual state contests matters, why we are concerned with the recurring impact of a New Hampshire primary. The Jackson candidacy does raise the question of whether black-white divisions remain an exception to this. But apart from that potential exception, this multiple and overlapping social base is also why different fields of candidates, mixing and matching old and new issues in different ways, make presidential politics appear so continually unpredictable. Indeed, seen from a different perspective, multiple and overlapping social divisions explain why any *individual voter* is differentially responsive to different issues and different candidates in different years.

In some systems, that sort of unpredictability would be restrained by strong parties, probably with further, fixed coalitions of organized interests within them. The U.S. nominating process, conversely, underlines the weakness of parties as determining organizations and the truncated and shifting nature of coalitions among the organized interests that are simultaneously components of and direct competitors to party. Parties no longer have much formal place in the nominating process, though the Republicans do remain nominally kinder in this regard. Even informally, it is *candidate campaigns*—evanescent organizations almost by definition—that serve as the basis for selection politics. To this is added multiple, segmented and fragmented, organized interests, which are encouraged to participate directly. While their participation itself is a further contribution to fluidity, even this is exaggerated by the way in which shifting *fields* of candidates, along with shifting issues of the day, can cause individual organized interests to be concentrated or dispersed in any given campaign and influential or irrelevant depending on the further drift of that campaign.

Finally, it is hard to imagine an institutional arrangement better suited to exaggerating these tendencies than the one that has evolved for presidential selection. The sequence of *open* participatory caucuses and *public* presidential primaries is ideal for reopening most issues in every presidential year—and often at several points within that year. As if that were not sufficient, the precise character and sequence of these institutions changes just enough from one quadrennium to another to guarantee some institutional arrangements with which no one has had any experience, producing some reliably unanticipated effects. And as if *that* were not enough, all of these institutions—participatory caucuses and public primaries—are crucially dependent on a low but shifting voter turnout, so that the electorate itself may have a noticeably different composition within the *same state* from one presidential year to the next.

A world constituted in this loosely structured and constantly shifting fashion—hence a world that must be *described* in this fashion as well—remains unsatisfactory at some visceral level for many scholars. Some of this dissatisfaction derives from the continuing absence of an ability to progress toward specific predictions. The personal identity of two nominees and then of one president, after all, is the consequential outcome—or so it perpetually seems—and being unable to generate a theoretical framework that will reveal these identities is thus inherently frustrating. More of this dissatisfaction, however, stems from a basic scholarly dislike of situations that are insufficiently specified and insufficiently explained. The same motives that drive an individual to attempt to develop a full and proper explanation of any social phenomenon, whatever those motives may be, fuel the desire to circumvent and push beyond apparently inherent limits on that explanation. Such a resolution presents possibly insurmountable difficulties, given the evolving description of the actual structure of presidential politics, because it involves the hunt for a dominant and unifying factor—ordinarily a *single* factor, often one overlooked by others. Nevertheless, each presidential election also resurrects this particular response to the underlying mission.

By far the best known of these—and also the most theoretically developed—is the "critical election" framework, hence the search for a "realignment" in the current election cycle. If this nominating contest were to conduce toward a classical realignment, for example, and if this general election were to confirm it, then something new and different—indeed, central and dominant—would be present on the landscape of presidential politics and should be reflected in scholarly accounts thereof. Books on a given presidential year now reliably include the search for this phenomenon, and 1984 was no exception. One or more chapters address the question in *The Elections of 1984* (ed. Michael Nelson); *The Election of 1984* (ed. Gerald Pomper); *The American Elections of 1984* (ed. Austin Ranney) and *Election 84: Landslide without a Mandate?* (ed. Ellis Sandoz and Cecil V. Crabb, Jr.) (all published 1985). The bad news—the title of the last of these really summarizes the conclusions of all—is that such a classical realignment has yet to emerge. Worse news would be a consensual victory for the view—the perspective known as "dealignment"—that U.S. politics has now changed sufficiently that such a critical, continuing shift is unlikely ever to recur.

That victory has so far not been conceded. In the meantime, other efforts to locate some dominant structuring factor outside the cluster of attitudes associated with partisanship and voting, also continue. Perhaps the most provocative of these for 1984 was Thomas Ferguson and Joel Rogers's, *Right Turn: The Decline of the Democrats and the Future of American Politics* (1986), an attempt to find the consistent, underlying explanation by looking at party financiers and "money men." The dilemma of most such works, however, is essentially the same: In a world where a shifting variety of factors clearly does make important contributions to shaping the ultimate outcome, efforts to argue against this conclusion end up, at best, by introducing yet another factor of some consequence into the equation. At worst and all too often, they are forced instead to *assume* that the concerns associated with their focus are the central concerns of politics and that matters unassociated with that focus are (therefore) unimportant; to believe, even then, that the behavior of their focal factor is principally as initiator and influencer rather than as respondent and reactor; and thus to ignore, in effect, the context in which their explanation occurs.

A less controversial effort along these lines, however, one distinctive in both argument and approach, is *News That Matters: Television and American Opinion*, from Shanto Iyengar and Donald R. Kinder. Presidential politics, by itself, is only a piece of what the book aspires to explain, so that much of its substance should be addressed under other headings. Yet the authors believe—and provide numerous ingenious experiments to assert—that a subtle but pervasive, deliberate but nonconspiratorial influence by television news is a central piece of what gives (all) U.S. politics its continuing structure. The authors find this influence in the processes of "agenda setting," which focuses public attention on certain matters, and of "priming," which establishes standards of judgment for them. In this, they differ very sharply from those who find the essence of media influence in either inherent characteristics of news gathering or in explicit "bias"—categories that would include William C. Adams, Emmett H. Buell, Jr., and David W. Moore from the Orren and Polsby volume, as well as Anthony Broh in *Horse of a Different Color.* To say that they differ from these others, however, is also to say that even their ingenuity and diligence cannot entirely overcome the standard dilemma of experimental research, namely, transferability to the real world. Thus at the final grand and global level they cannot escape, for example, the problem of differentiating pure media effects, that is, those flowing from *press decisions* about coverage, from agenda setting and priming, which are only the by-product of *press coverage* of real acts and conflicts among major political actors. What they end up doing, then, in line with most others in this field, is adding yet another element to the array of those which certainly do shape the politics of presidential selection.

Is that, accordingly, as far as scholarly analysis can go? There are some basic notions that are fundamental to understanding the politics of presidential selection, in overlapping and cross-cutting cleaveages, in weak and segmented intermediaries, and in semi-independent and competitive institutions. While these both advance and limit an understanding of the resulting politics, it is possible to add further specific elements *and* to explicate them systematically—as Orren and Polsby, Broh, Reiter, and Heard and Nelson have certainly done. Efforts to go beyond this with a dominant alternative, on the other hand, as with numerous searches for a realignment, have been unsuccessful to date. Indeed, even a distinctive effort like that of Iyengar and Kinder ultimately provides only further introduction of an additional factor shaping the politics of presidential selection. Nevertheless, when all these studies are melded into the continuing *collective* scholarly effort on the topic, some additional, recurring outlines may be emerging. These build on emerging regularities in mass participation and in the dynamics of (elite) campaigns to produce a recurring factional structure. If these factions remain somewhat fluid in terms of their programs and their constituencies, they still help to constrain vague assertions about "shifting coalitions" and "programmatic flux."

An underlying pattern to the turnout in presidential primaries and party caucuses is in fact increasingly apparent, though by itself it is a very blunt instrument of prediction. Those who do come out to participate, in forums that vary in aggregate turnout from an occasional 40% or above at the high end to an occasional 5% or below at the low, tend disproportionately toward one of two categories. Either they are the most highly educated and attentive, those most likely to get there on their own, or they are the best organized (among the supporters of their party), those most fully integrated into the *organizations* likely to participate in nominating politics. This tends to divide these participants additionally, within each party, between those who are attached to the national party program as well as to its main organized interests on the one hand and those who are self-consciously independent of (and sometimes alienated from) that same policy coalition on the other—into an "establishment wing," which includes the official party and the major interests associated with it, versus a "dissident wing," which is reliably made up of those who oppose "politics as usual" and who believe that "the special interests" (by which they ordinarily mean the establishment wing) are too influential for the public good.

The dynamics of nominating politics, as they have come to be understood, are tailor-made to convert these incipient divisions in the mass (participating) public—quickly—into campaigns centered on a specific standard-bearer for each wing. In a nationwide sequence of open primaries and the occasional key caucus, very few candidates will (indeed, can) begin with nationwide name recognition and strong voter attachment. Most candidates will be profoundly local, in the sense of not being from (or known within) the opening states. They must thus triumph in one or more of these early contests or never move to national prominence at all. The exceptions, those candidates who *are* lucky enough to begin with national name recognition, still ordinarily begin with a broad but shallow public attachment. They, too, must succeed early or fall by the wayside, though they can potentially weather an early loss or two, unlike the true unknowns. What is additionally crucial, however, is that these two inherent, structural categories of candidates tend to coincide with the divisions within the participating mass public.

That is, those in the second category, of broad but shallow initial support, are more likely to be from the establishment wing of their parties, having built up their national name recognition through an extended career in national politics and having acquired the ties to dominant organized interests that go with that career. Those in the first category, conversely, of little or no national name recognition, tend to be from the potential dissident wings, else they would already have been crowded out by those (establishment) figures with their extended careers and national ties. This, of course, explains why it is harder to gauge the identity of dissident standard-bearers than of establishment champions in advance. More to the point, however, these simple factors—this gross outline of mass participation and this crude summary of elite dynamics—explains not only why there is a recurring factional structure within each party, one that belies some of the appearance of flux and disconnection, but also why the two parties are roughly mirror images of each other.

At bottom, ordinarily, there are *two* initial contests within the nominating politics of *each* party, with certain reliable contours to them. There is the contest to secure leadership of the establishment wing of the party, and there is the contest to secure leadership of the dissident wing. Occasionally, one candidate can blend support so successfully—or one contest is effectively won by so dominant (or weak) a contender—that this settles the nomination. But ordinarily two factional leaders emerge and then duel for that nomination. Indeed, it is possible to put some detail into their programmatic positions, too. The division between establishment and dissident in fact tends not to be over economic policy, where Republican contenders are reliably conservative, Democratic contenders reliably liberal. Instead, the division tends to be over cultural and life-style issues—over what are known in the U.S. as "social issues"—where establishment contenders tend to be moderate, while dissident champions lean toward the extremes (left for the Democrats, right for the Republicans). Finally, it is possible to specify this factional situation even further by noting that it is not reliably a 2-faction but rather a 2½-faction contest, again in both parties. Or at least the possibility of a secondary conservative insurgency within the Democratic nominating contest or a secondary liberal insurgency within its Republican counterpart is still present, though these are no longer reliable occurences and do not reliably attain the level of expression that they still secure in congressional elections.

The pattern to these factions is not sufficient to provide hard and fast, prior expectations for the precise twists and turns of nominating politics. Yet this factional structure does help additionally in explaining an even larger element of regularity in presidential politicking, the process by which individual contenders for the nomination can take on the dominant concerns of the mass public during the course of nominating politics—can *find* both their message and their constituency along the way, being themselves reshaped into something rather different from what they may originally have been. Factions, accordingly, provide a way of making some further sense of new presidential contests, a sense larger than the usual "If X does well here and Y does well there, then this will happen, but if Y does well here and Z does well there, then *that* will happen"—an inherently unhappy form of (non)explanation. On the other hand, even the use of recurring factions merely gives shape to—and constrains—the continuing fluidity in presidential politics.

The background to that politics—multiple and overlapping social divisions, weak and truncated intermediaries, and an institutional structure ideal for forcing constant renegotiation—is still its dominant factor. It would be too much—and far too uncomfortable—to call the resulting malleability the "genius" of U.S. politics, since that very malleability also makes every analyst nervous from time to time. Yet to call it the "essence" of that politics may not be too strong. As a result, a scholarly community working on that politics will still have to attend principally to this background, by isolating significant pieces and giving more systematic attention and elucidation to them. In that way, this scholarly community should continue to make a collective contribution to understanding, by continuing to respect the essential character of the phenomenon under study.

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POLITICAL THEORY

Critical Encounters: Between Philosophy and Politics. By Fred R. Dallmayr (Notre Dame, IN: Notre Dame Press, 1987. xi, 296p. $28.95).

Reviewing this book presents me with a delicate task. The subtitle is (purposely) the title of my recent book, and the introduction pointedly addresses my claims about contemporary academic political theory's estrangement from the particularities of politics and about the problems associated with its absorption in issues defined by academic philosophy. I have great respect for Dallmayr's work, and I share many of his theoretical interests and assumptions. But as he recognizes, this volume is quintessentially representative of the genre that I criticized in *Between Philosophy and Politics: The Alienation of Political Theory* (1986).

The collection of 11 essays, with the possible exception of those devoted to Nietzsche (as interpreted by Jacques Derrida and Heidegger) and Theodor Adorno (approached in terms of Heidegger), consist of critical explications of the work of contemporary academicians in the fields of social and political science and philosophy (Jürgen Habermas, Karl-Otto Apel, Hans-Georg Gadamer and Jacques Derrida, Richard J. Bernstein, Alasdair MacIntyre, Michael Theunissen, Anthony Giddens, and Niklas Luhmann).

There are no grounds for disagreeing with the prepublication assessments that Dallmayr is a "trustworthy interpreter" who is "very much in touch with the principal currents of contemporary thought"—Anglo-American and Continental—and who "has the extraordinary ability to engage in sympathetic critical dialogue" and to communicate "the perspective of those he interprets with . . . clarity and charity." With respect to authors such as Theunissen and Luhmann, who have been less accessible to an English-speaking audience, the accomplishment is particularly valuable. The reviewers, however, wish to stress that more has been accomplished than faithful rendition embellished by critical analysis. Dallmayr's own position "becomes more sharply defined," and "his own voice reaches a new level of definition."

Dallmayr is sensitive to suggestions that his past work has been marked by "a talent for empathy" that has "virtually submerged [him] in interpreted texts" and that was "thus purchased by lack of a clearly defined 'standpoint'" (p. ix). He notes, however, that this project does not "focus" on producing such a standpoint; even though the essays, like his other recent work, are the product of his own attempt "to chart a course leading from critical phenomenology (or critical hermeneutics) in the direction of what [he] came to call 'practical ontology'"—through a context of "critical theory, transcendental hermeneutics and neopragmatism" (p. ix). He is especially concerned with issues generated by varieties of postmodernist philosophy and with the implications for social thought of such philosophical events as the "decentering of the subject" or the loss of the idea of an autonomous individual as the basis of action and judgment (p. 2).

What Dallmayr's essays have to do with politics, except in some very abstract or historically remote sense, is consistently opaque. He claims to deal with authors "whose arguments capture and pinpoint the salient issues of our age"; but those issues turn out to be philosophical in the narrowest sense of that term. It may be true that "the intellectual [maybe more precisely, 'academic'] agonies of our time are unintelligible without attention to phenomenology, hermeneutics, linguistics, critical theory, deconstructionism, and various forms of pragmatism" (p. x). But the "agonies" discussed and the "agonism" practiced by Dallmayr are only by occasional allusion connected to concrete past and present forms of political thought and action.

A prominent political theorist once responded to this kind of criticism by saying "after all we are philosophers, and it is hardly our task to say how much money a welfare mother should receive." One may agree—but not necessarily for the reason implied. The problem, for example, of how to manage solid waste may be beneath the dignity of philosophy, but it may also be beyond its competence and authority. What exactly has philosophy to do with politics? The answer need not be "nothing"; but the problem, both descriptively and normatively, is a complex practical and historically situated one that depends on the character of both politics and philosophy. It does not admit to a general philosophical solution. Try to explain to a person providing support services for AIDS patients or someone working on agricultural development in an African village that the agonies of our age revolve around the decentering of the subject.

Dallmayr's notion of "ontological praxis" may suggest something paradigmatically oxymoronic, but it is intended to convey an idea of philosophy with a substantive and practical bent. His claim, however, that this kind of "lived engagement" is not "removed from 'real-life' occupations" and does not merit "the verdict pronounced by John Gunnell on most versions of contemporary social and political theory" (p. 3) is less argued than asserted. He employs my words to indicate political theory's problematical situation when he refers to his essays as inhabiting "the curious demimonde" between philosophy and politics. But his critical encounters do not take place in that realm. They move quite distinctly within the conceptual world of professional academic discourse and among arguments that are for the most part equally distanced from political life.

Dallmayr incorrectly claims that I presuppose a "neat segregation between philosophy and politics," present them as "opposed to one another (along a scale of authenticity)," and intimate "having access to, or being in command of, a basically unalienated, authentic, or genuine mode of political discourse" (pp. 3–4). This kind of misreading prevents bridging the issue that divides us. I posed no such necessary separation or invidious comparison but rather stressed the practical fact of distinction, the tendency of political theory to assume that philosophical discourse is in some way identical with political discourse, and its failure to confront its own situation and the existential complexities of the relationship. My claims about the alienation of political theory were not predicated on access to any privileged discourse. On the contrary, my most persistent critical argument focused on the manner in which various forms of academic political theory have looked to philosophy for a basis of transcendental political judgment.

It may well be that political theory as a professional academic pursuit should no more be expected actually to speak to and about politics than, for example, the philosophy of science speaks to and about the everyday activity of science. Its alienation is less a function of its differentiation and esotericism than of its pretension to both practical and theoretical involvement. *Wir Gelehrten* need not be ashamed of who we are, but we must reflect authentically on our identity and its possibilities.

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Feminism and Political Theory. By Judith Evans et al. (Beverly Hills: Sage, 1986. vi, 155p. $28.00, cloth; $12.95, paper).

Feminism and Political Theory, a group of Political Studies Association conference papers, exhibits the strengths and weaknesses of the conference paper genre. On the one hand, the reader gains a sense of what passed for state-of-the-art thinking on a particular delimited topic at a given time and place and in light of an articulated agenda. On the other, essays of this sort are often highly condensed, with assertion frequently substituting for fleshed-out argument, and quickly dated. This is especially true in a field that is as rapidly developing and immodestly encompassing (purposely so) as feminist political theorizing. A volume dedicated, as this one is, to a specific political purpose—legitimizing feminism as a subject of study in British political science—may also lose, over time, some of the energy that initially made the subject matter exigent.

A warning, then: do not expect too much. If the reader is looking for new approaches or provocative alternatives to either mainstream (or "malestream" as several authors insist on putting it) theorizing, or by-now established feminist theorizing, he or she will be disappointed. If, however, the reader approaches the volume to gain some sense of how various debates are couched within British feminist academic circles, and how texts by U.S. feminist writers, academic and nonacademic, are, or have been, appropriated within the British milieu, the essays offer helpful summaries and provide a decent map to the discursive landscape.

All the authors share an animus against the New Right, not so new anymore and never as cohesive an entity—either here or in the United Kingdom—as the contributors suggest. All oppose liberalism, claiming that it has "little to offer the feminist cause in that its values of autonomy, tolerance and the preservation of the private realm lead to a contradiction which becomes for feminism an impasse." And, as the quotation in the previous sentence indicates, all yearn for a world in which tensions are eased and contradictions eradicated. None questions whether this is or should be an aim either of politics or theorizing about politics.

Judith Evans, in her essay, "Feminism and Political Theory," makes, but does not defend compellingly, the claim that "the essential techniques of political theorists are value neutral," a curious avowal that locates "bias" in the assumptions of theorists but not in "techniques which they employ." What these "techniques" are and whether there is some generic technique of the value-neutral sort shared by all political theorists is never clarified. What appears to be at stake here is Evans's desire to distance herself from a feminist viewpoint she opposes, one that "demands the dethroning of logic and reason." As there is no serious feminist theorist I know who promoted decapitation of the thinking portion of the body, human or political, her attack is oddly without a target save for thinkers who are quite marginal to serious feminist political theorizing. Her larger point—that feminist theorizing deserves to be considered by political theorists more generally is, however, well taken.

Ursula Vogel details the different strategies for women's liberation that emerge from what she tags the rationalist and romanticist traditions. Vogel leaves open the possibility that a thinker might "acknowledge specific, gender-based characteristics of reason without thereby implying the superiority or inferiority of one against the other." Her subsequent defense of Friedrich Schlegel's celebration of Greek exemplars of strong womanhood, including Spartan women, against Rousseau is a bit odd, however, in light of the fact that Rousseau valorizes the Spartan mother who loses five sons in battle but gives thanks to the gods for Sparta's victory, as the exemplary female citizen. Why Schlegel's exaltation of Spartan mothers is recommended to feminist thinking but Rousseau's is anathema is unclear. Karen Hunt takes a look at "the socialist construction of women's politicization" with especial attention to Britain's Social Democratic Federation and offers a useful historic and rhetorical summary of debates internal to socialists about the "woman question." "Those of us who are socialists and feminists," she insists, require "a more analytically persuasive means of cementing the two analyses."

Tessa ten Tusscher, in a short piece on three big topics—patriarchy, capitalism and the New Right—throws down the gauntlet with abandon, claiming that "feminist political theory is only of value if it is of use to the political project of ending women's subordination." In an essay attacking the New Right for its narrow moralizing, ten Tusscher's tendentious approach to thought is troubling and very much a discursive double to what she opposes. She and the New Right, as she depicts it, enlist ideas and give them their marching orders with specific aims in mind. Ideas that do not serve are to be jettisoned. But she does offer entrée into the British debate over Thatcherism for the uninitiated.

Georgina Waylen, in "Women and Neo-Liberalism," targets neoliberalism as "the antithesis of the ideas of the women's movement," with Milton Friedman and Frederich Hyak singled out as strong exemplars of a mode of thinking that leads to "the intensification of the subordination of women." Evans weighs in with a second contribution on "Feminist Theory and Political Analysis" that urges total abolition of the public-private distinction, without bothering to look at potentially problematic implications. One U.S. example: in the United States abortion rights flow directly from the assumption of a zone of privacy. *Roe v. Wade* builds on *Griswold v. Connecticut*. Finally, Elizabeth Meehan's survey, "Women's Studies and Political Studies" is rather dated in terms of the literature she draws upon, but she does usefully encapsulate debates over whether women's studies should be an integrated, or a "separate," enterprise.

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The Philosophical Discourse of Modernity: Twelve Lectures. By Jurgen Habermas. Translated by Frederick Lawrence, with an introduction by Thomas McCarthy (Cambridge: MIT Press, 1987. xvii, 430p. $27.50).

The phalanx of critical modernity has taken up its position on the beach, engaging the forces of postmodernity as they swarm ashore. The threat must be met and driven back to its barbarian homeland. The rather staid title of this impressive book belies its drama, polemics, and philosophical power. We are lead on a direct charge into the heated issues of modernity and postmodernity. The difficulty of surveying such a significant work in a brief review is eased somewhat by Thomas McCarthy's excellent introduction which lucidly summarizes the key themes. I will thus only give a sense of the overall thrust of the book from two perspectives: its place within the context of Habermas's work as a whole, and its attack on those who threaten the "discourse of modernity."

With the translation of this book, as well as that of the second volume of *The Theory of Communicative Action* (1988), the project Habermas has been pursuing since *Knowledge and Human Interests* becomes fully available to the English-speaking world. Its theoretical heart is a "communicative" conception of reason and action that claims to be distinctive enough to (among other things) withstand the various critiques leveled against reason in its modern form: the reflective calculations of the individual or collective subject. The communicative model claims to be able to perform this feat because it grounds itself in neglected potentialities of the life-world structures of modern culture, and yet also generates a perspective from which modern society and its prevailing conception of reason can be critically assessed. Habermas thus offers us a way of understanding modernity that is critical but that nevertheless develops its tools of critique from the materials of modernity itself. This ambitious undertaking is carried out in *The Theory of Communicative Action.*

Implicit in such a task, however, is a rejection of attempts to generate more radical distance from modernity and its dominant concepts, attempts that can be loosely gathered under the umbrella term *postmodernity.* Habermas leaves no question here as to who his main opponents are: Derrida and Foucault and their forerunners, Nietzsche and Heidegger. Each claims to have developed a mode of thinking that somehow lifts their critical perspectives out of the "discourse of modernity" (pp. ix, 74), something they see as necessary to getting free from the entanglements of modern subjectivity and its attendant form of reason. Against this position, Habermas sees his goal as twofold: first, to reconstruct the philosophical discourse of modernity historically so that both his own position and that of the radical critics can be better understood and evaluated and second, to show that these critics do not achieve what they intend.

The philosophical discourse of modernity starts, according to Habermas, with Hegel, not because he is the first modern thinker, but because he is the first to take modernity as the central theme of philosophy. This thematization takes the form of asking how modernity can achieve "self-reassurance" (p. 16). The question becomes pressing when, in the wake of the Enlightenment, the key principle of subjective reason—which brings the achievements of modernity—increasingly falls under the suspicion of being corrosive of the unity of any possible form of social life. The problem, then, for modern philosophy is to show how to combat this corrosiveness and yet do so in a fashion that does not regress behind modernity, that is, in a fashion in which modernity still "creates its normativity out of itself" (p. 7). Hegel's solution to this problem—absolute subjectivity—is found wanting; but Habermas takes as definitive his statement of the central "theme" of the discourse of modernity, as well as of the "rules within which the theme can be varied" (p. 51). The bulk of the book surveys the ways in which the radical critics have tried and failed to bid farewell to this discourse. In the end, their approaches simply do "not seriously lead beyond the philosophy of the subject." That breakthrough can only be achieved by a decisive shift from "the paradigm of consciousness" and subjectivity to "the paradigm of mutual understanding" and intersubjectivity, a shift that at the same time respects the theme and rules of the discourse of modernity (pp. 74, 296).

Given the level of analysis and the chosen "rules" of discourse, Habermas's rebuttal of the critics is persuasive. Yet one feels just a bit uneasy gloating on the beach with the victorious Greeks. Are there perhaps ways of engaging these critics in which they are less easily disarmed? Let me simply allude to one. As is made clear in Habermas's discussion of Derrida, the communicative model of reason and action emphasizes the action-coordinating function of language over the "world-disclosing" function stressed by the critics (pp. 190–210, 329–35). Among other things, Habermas suggests that systematic social theory and political philosophy will not be plausible if we take the critics' position. In this I think he is essentially right. But what does one then do with the world-disclosing dimension of language? Elsewhere, Habermas has tried to assign this dimension a kind of derivative status, but the attempt is rather unpersuasive, (as is the reverse strategy of the critics).

My underlying concern can now be stated as a question: What would it mean for Habermas, political reflection, and the discourse of modernity as a whole to give neither dimension of language such a strongly preeminent status? In order to think this question through, we may have to invite the barbarians ashore again.

STEPHEN K. WHITE

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Power and Marxist Theory: A Realist View. By Jeffrey C. Isaac (Ithaca: Cornell University Press, 1987. x, 238p. $24.95).

As much as political scientists bemoan the quagmire called the power debate, they keep coming back and asking epistemological and ontological, if not methodological, questions. It has become de rigueur that each study of power also be a study of how to study power. This book is no exception and even offers a spirited defense of the necessity of such self-reflective work, especially in the case of power. This is an ambitious book that seeks to pose a serious challenge to the existing state of affairs in the debate over power. Its goal is nothing less than showing how all the combatants in the "three-faces-of-power" debate share a consensus on a Humean view of causality, a nomological understanding of science and an empiricist concept of power. The power debate is trapped in empiricism; and as long as it is mired there, it will not be able to make much sense of power. In lieu of empiricism, "realism" offers a better possibility for establishing a science of power, and Marxism constitutes just such a realist approach to power.

Realist science seeks to specify the real character of things in a way that informs us of their enduring capacities. It does not assert the claim that it can specify the character of things in an absolute sense or that the real is directly experienced as things that exist independently of the knower. It also stresses the idea that while things possess enduring properties, things never exist in their pure form and that the extent to which they exhibit these characteristics is contingent upon how they play out in the real world in any particular instance and at any particular time. Realist science, however, does emphasize that the world consists of things rather than events. From this perspective, science is concerned essentially with what kinds of things there are and is only derivatively concerned with predicting whether events will happen. And while it recognizes that theories may be *under*determined by facts, it stresses that they are not *un*determined by them. Marxism is a realist science in that it seeks to specify the enduring properties and character of capitalist regimes per se, recognizing that such regimes will take various social formations in various historically specific circumstances.

Realist social science differs from its empiricist counterpart especially as exemplified by the problem of power. Empiricism focuses on behavioral regularities to the neglect of structure. For instance, all parties to the debate over power fail to account for how structure implies the capacity to exercise power over one who assumes a certain role within society—be it master or slave. In what is bound to be a controversial analysis, Isaac shows how even scholars like Steven Lukes, who argue for taking structure into account as "the third face of power," fail to specify how structure entails power because they are trapped by empiricism into seeing power in strictly behavioristic terms. Lukes emphasizes behavior over structure because power is a concept of human agency and structure implies determining limits on action. Relying heavily on the work of Anthony Giddens, Isaac suggests that the distinction between structure and agency is overdrawn. Instead, we should recognize "structuration," or the dialectic process by which structure creates the potential for agency and agency renegotiates the conditions for structure.

Empiricism promotes a behavioristic, sequentially causal view of power as "power over" to the neglect of "power to," as in the enduring potential or capacity inherent in a social role. Just as realist science studies enduring capacities of things, the realist concept of power focuses on the potential for, and limits on, power inherent in any social role. The ability to realize such "relational" power is contingent on how social actors negotiate within their structured circumstances. Structure creates the potential and limits for the exercise of power, and agency reproduces and remakes structure. Only such a realist view of power can account for structure and agency and get us beyond the limitations of the power debate in its current state.

For Isaac, Marxism as a realist science offers just such a theory of power; and he tries to convince us of that by reviewing debates over Marxism on power, class, and the state. In the process, he deftly defends a realist Marxist perspective on power from a diversity of criticisms inside and outside of Marxism. There is much to interest readers here in thorough reviews of the debates about the deterministic character of Marxist structuralism, the meaningfulness of class as a way of understanding human struggle, the greater utility of the "state" as compared to "government" for understanding power, the character of the capitalist state, and poststructural criticisms of the totalizing and reifying tendencies of Marxism. What emerges is a nonreductionist Marxism that forgoes economic determinism and recognizes that power relationships grow out of the structure of society. While some of this gets far afield of the original problem of the debate over power, in the end a contemporary neo-Marxist understanding of power comes through effectively.

This is an important book. It is a thorough, readable, and scholarly work that raises fundamental questions about the contemporary power debate and in so doing poses a meaningful alternative that will not be easily dismissed.

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The Critique of Pure Modernity: Hegel, Heidegger, and After. By David Kolb (Chicago: University of Chicago Press, 1986. xvii, 316p. $25.00).

During the past decade the currents of European postmodernist thinking have slowly worked their way into the U.S. academic world, initially through the humanities and more recently in the social sciences, with political science one of the last to be affected. Meanwhile in U.S. philosophy, a native but similar movement, antifoundationalism, has grown up as a speculative alternative to traditional metaphysics and to nonmetaphysical analytic philosophies. The two tendencies were brought together in Richard Rorty's *Philosophy and the Mirror of Nature* (1979), which has become the prime focus for discussion of U.S. postmodernism. Now David Kolb, professor of philosophy at Bates College, has presented his contribution to the project of "overcoming modernity," following in the antifoundationalist line but drawing upon continental European thought as his subject matter.

Kolb's commentary on Hegel and Heidegger and his own theory of modernity are of interest to political theorists and to reflective political scientists in specialized branches of the discipline because they center on the possibility for constituting a coherent way of common life under contemporary conditions. Both Hegel and Heidegger, according to Kolb, try to get beyond the standard Weberian description of modern society as a conjuncture of vacant willing subjects standing over against a neutral field of fact by enfolding human beings into a finite context. In Hegel's case that context is the circular motion of spirit running through its various dialectical moments; and in Heidegger's it is the "propriative event," that is, the occurrence in which man and beings are gathered together in distinctive relations that constitute historical epochs. Kolb finds both German thinkers to be inadequate, but on different grounds in each instance. Hegel is successful in combining multiplicity and unity on the level of speculative thought, but he is unconvincing in demonstrating a tight relation between his dialectic and the various details of historical development. Heidegger, in contrast, provides too much substantial unity in his account of history, destroying continuities by setting up hermetic epochs constituted by invariant relations of man to beings. Both, however, have the same worthy project, which is to reduce the isolation of modern subjectivity without returning to the relative deindividuation of traditional societies.

Kolb's commentaries are done carefully and responsibly, evincing conversance with contemporary scholarship and a broad and fine understanding of the original works. He presents a strong, though not thoroughly convincing, picture of Hegel as a transcendental philosopher, rather than a metaphysical idealist, and he gives one of the more lucid and accessible analyses of Hegel's logic in the literature, showing how it makes the political works intelligible. But the high point of the commentaries comes in his elucidation of Heidegger's term *das Gestell,* which refers to how beings are "presenced" to man in the current technological age. Here Kolb shows his virtue of clarity to best effect, making difficult and significant texts readily understandable without sacrificing subtlety. The three chapters on Heidegger, which emphasize his late work, offer a gateway for political scientists unfamiliar with postmodernism to grasp some of its basic insights.

The strengths of Kolb's discussion lie with his skill as a lucid and responsible interpreter and scholar. He has mastered his subjects in a technical sense, attending to richness and variation in meaning, disentangling complexities, and keeping the wider structure of thought in mind as he works through each part. But his power of mind is not matched by his sympathy and depth of understanding, particularly when he encounters Heidegger. Indeed, he fails to grasp how seriously Heidegger took the turn in modernity through which "everything faces everything else as ready for ordering and use," and "men too belong as standing reserve ready for human engineering" (p. 147). Here, in the thought that "every entity is encountered as open and pliable for an ordering that proceeds without any center from which an ordering force radiates" (p. 147), Heidegger anticipates such poststructuralists as Jean Baudrillard, who argue that our activities and meanings have lost reference to any primary reality. Heidegger found "universal imposition" to be a cause for profound concern, but Kolb does not even seem to sense that concern.

Kolb's lack of depth and, indeed, seriousness comes clear when he offers his own pluralistic and antifoundationalist interpretation of life, which does not overcome modernity but loosens it so that experience resembles William James's pluralistic universe. Without any hint of ground or justification Kolb expresses the democratic and reformist sentiments typical of the U.S. pragmatic tradition. Political scientists will recognize his description of life as a naive version of the one Arthur Bentley presented in *The Process of Government* eighty years ago—only now we are playing a "game" which we must be sure not to take "perfectly seriously" (p. 251). Kolb's commentaries are admirable in a scholarly sense, but he failed to gain enough substance from them.

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Human Rights and Human Diversity: An Essay in the Philosophy of Human Rights. By A. J. M. Milne (Albany: State University of New York Press, 1986. i, 186p. $39.50, cloth; $12.95, paper).

Locating a value-neutral basis for a concept of human rights and then defining the minimum essential rights within the concept may be an insuperable task. This frustration arises from value preference (bias), from lack of cultural relativity, from disregard for advances in science and understanding, and from proneness to deductive-prescriptive methodology. Milne slashes at these veils with the earnest iconoclasm necessary to make a real advance in the literature. The result is a coherent basis and architecture for conceptualizing human rights. This achieved, others may build upon Milne's concept in the operational sense for the tangible betterment of human rights. In these respects, Milne's essay merits serious reception among the disciplines of law, philosophy, government, international relations, anthropology, sociology, and comparative religion–comparative ideology.

Milne begins his search for the basis of human rights with Kant's humanity principle but finds that it suffers from the same perceptual frustrations mentioned above. He therefore grafts upon Kant's humanity principle two moral principles of his own ascertainment. These are the moral principles of beneficence (it is natural to choose good over evil) and of practical reason (which requires that like cases always be treated alike, with differentiated treatment only where there are relevant differences). These two principles constitute the basis of a "common morality which is not further reducible and permit the concept of community to be actualized. Owing to contextual circumstances, each of these communities (the term is taken broadly) evolve their own 'particular morality' " (p. 82). It is through this particular morality of communities that the irreducible common morality attains its diversity. It is precisely this diversity, according to Milne, which must be taken into account when resynthesizing the minimum human rights located in common morality. Milne finds these resynthesized human rights located in common morality to be the right to life and the right to justice in the form of fair treatment, these being morally prior to all the rights which follow. Following from the right of justice in the form of fair treatment are the rights of fellowship (the right to aid), of social responsibility, of freedom from arbitrary interference, of honorable conduct, of civility and of child welfare. Milne sees no possibility of community without these, only the sort of poor, nasty, and brutish life possible in a Hobbesian state of nature.

Milne's contribution to the multidisciplinary literature pertaining to human rights is his rigorous explication, in terms of philosophy, of the content of each of these resynthesized rights. In this he strives conspicuously and admirably, because he is trailblazing, to avoid philosophical self-impeachment. Transcending wishfulness, Milne resorts with rationally persuasive results to the philosopher's inductive methodologies, enhanced by falsification techniques. To this he adds somewhat awkwardly the right-obligation correlation analysis articulated early in this century by the English legal philosopher Hohfeld. Further in the name of cultural relativity, Milne disengages his common morality–particular morality construct, and also his methodology, from any system-specific moral precedent. In this his product does make a claim of universality (common morality) and of verifiability. He is soberingly persistent in comporting human rights philosophy with the empirical "is" of the human experience, through time and across value systems.

Further thinking about Milne's work requires refinement, however, of two operationally distinct but conceptually linked questions not squarely confronted in this expressly limited essay. First, who actually sets the norms or operationalizes morality within communities (and within nation-states)? Stated simply, who articulates particular morality and by what process is this accomplished? Secondly, what is the source and function of revolution within communities (and within nation-states)? Where Milne does approach these questions he assumes a subjunctive style and a level of marked abstraction. These two still open questions in no way represent the self-falsification of the construct Milne has undertaken but indicate instead that the complex task has just begun. Indeed, the key to norm articulation and to the source and function of revolution likely may be found to lie in Milne's principles of common morality, that is, in beneficence and practical reason, taken together. It is not sufficient, however, simply to array norms without ascertaining the process by which they are formulated (question 1) and adjusted (question 2). Further and definitive work on the universal human rights standard is called for, and Milne has laid a valuable basis as a starting point.

EUGENE D. FRYER

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Gender and History: The Limits of Social Theory in the Age of the Family. By Linda J. Nicholson (New York: Columbia University Press, 1986. 238p. $29.00).

Feminism and Equality. Edited by Anne Phillips (New York: New York University Press, 1987. 202p. $35.00, cloth; $15.00, paper).

Gender and History and *Feminism and Equality* are strikingly complementary books. They both concern the relationship between feminist theory and liberal political thought.

Linda Nicholson denounces the public-private distinction that lies at the core of traditional liberal theory but insists that Western culture's understanding of what is private and what is public has changed significantly between the seventeenth and late twentieth centuries. The heart of the book is part 2, which argues that liberalism, Marxism, and feminism each embodied different understandings of the nature of, and the relationship between, the private and the public, understandings that in turn reflected different historical conditions. In Nicholson's view, liberalism reflected the emergence of "a new form of social organization wherein the family and the state as we now comprehend them were being created out of the older institution of kinship." Marxism, in turn, reified the "growing separation and dominance of the sphere of the economic" and "ignore[d] the historically contingent nature of the separation of the family and the economy" (pp. 2–3). Political feminism emerged when a realignment between public and private led to an increasing "individualization of social life" and "the replacement of the household by the individual as the basic political unit" (p. 4). Only by a careful attention to history, Nicholson argues, can feminism avoid the universalizing tendencies of both Locke's and Marx's thought, tendencies that undercut the accuracy and usefulness of those theories.

Nicholson's attention to the treatment of kinship, family, economics, and politics in Locke and Marx is intelligent and stimulating. Nicholson effectively criticizes past theory and demonstrates the necessity of historical consciousness in political theory but leaves to others the construction of a political theory that will take account of the history of gender relations.

Anne Phillips's collection of essays, *Feminism and Equality,* raises the kind of questions any adequate future theory will have to answer. All the essays have been previously published, but Phillips has selected and organized them with great intelligence so that they speak to and illuminate one another in remarkable fashion. (In a serious oversight, however, the authors are nowhere identified beyond their names, leaving the reader to guess their nationalities, backgrounds, institutional affiliations, if any, and so forth.)

The questions these essays raise fall roughly into two categories: What is the relationship between equal rights and equal opportunity for men and women and What does equality mean in a world of sexual difference?

Juliet Mitchell's "Women and Equality" and Michele Barett's "Marxist-Feminism and the Work of Karl Marx" point out the links between liberalism and feminism and defend the continuing importance of attention to equal rights even as they criticize liberal theory's inability to account for economic and sexual oppression or liberation. Bell Hooks's "Feminism: A Movement to End Sexist Oppression" is less patient. Hooks, a black theorist and activist, wants to redirect the attention given to "rights" to systems of economic, racial, and sexual oppression.

Zillah Eisenstein's "Elizabeth Cady Stanton: Radical-Feminist Analysis and Liberal-Feminist Strategy" and Ellen DuBois's "The Radicalness of the Women's Suffrage Movement" use examples drawn from the nineteenth-century suffrage movement to show that liberal theory in the hands of dedicated feminists forces a thoroughgoing revision of the public-private distinction and of our understandings of both family and civil society. Carole Pateman's "Feminist Critiques of the Public/Private Dichotomy" shows how the public-private distinction masks a patriarchalism which is an "essential, indeed constitutive part of the theory and practice of liberalism" (p. 109). One of feminists' major tasks will be to show how it is that "although the personal is not the political, the two spheres are interrelated, necessary dimensions of a future, democratic feminist social order" (p. 120).

Jean Bethke Elshtain's "Against Androgeny," Sally Alexander's "Women, Class, and Sexual Difference," and Denise Riley's " 'The Serious Burdens of Love?' Some Questions on Childcare, Feminism, and Socialism" focus attention on the sexual differences between men and women. Given those differences, they ask, what would real equality between men and women look like? They also ask whether, as Alexander puts it, "antagonism between the sexes is an unavoidable aspect of the acquisition of sexual identity." If so, then while the organization and meaning attached to gender can differ markedly, personal and political tension stemming from sexual difference is inevitable (p. 173).

Feminism and Equality is a splendid introduction to the range of feminist analysis concerning equality and gender difference. It shows conclusively that equality cannot be reduced to equal opportunity in the "public" realm and raises all the right questions about the implications of sexual difference for future feminist theory.

MARY LYNDON SHANLEY

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New Ways of Knowing: The Sciences, Society, and Reconstructive Knowledge. By Marcus G. Raskin and Herbert J. Bernstein (Totowa, NJ: Rowman & Littlefield, 1987. ix, 293p. $34.95, cloth; $15.95, paper).

For most political scientists the focus of interest in this multiauthored book will be chapter 3, "Toward a Reconstructive Political Science," by Raskin and Bernstein. The essential message of the book is in this chapter. Although the other chapters assume viewpoints and contain analyses that many social and behavioral scientists might find interesting, most political scientists will find in chapter 3 and the introduction by Bernstein as much as they might profitably invest in time and attention.

From the outset it is evident that the book is not really about "new ways of knowing"; the authors approve old ways (those of traditional societies) and also advocate ways already adopted in some fields of modern scholarship. I found none new to me. The book is a contribution to "liberation" scholarship—*liberation* meaning freedom from the biases of class, race, gender, and technocracy that are alleged to have thoroughly dominated science.

The authors declare that "reconstructive knowledge is itself a myth for generating social reconstruction and teasing out implications of present dominant modes of thought" (p. 100). Reconstructive knowledge, say the authors, is political. It seeks to decode the various languages of classes and specialties to show their values and premises (p. 88). But neither method nor finding appears to constitute a new form of inquiry. Students of semantics; of epistemology; of the history, philosophy, and sociology of science; and even of political science in the conventional sense have been at this analytic form of inquiry for a long time and clearly not always in the service of a dominant socioeconomic paradigm. The real political science of the twentieth century, say the authors, is "comprised of political physics, political agronomy, political biology, political economics, as well as a very political political science." The authors hope that "a reconstructive political science in all its branches will come to replace the present enterprise" (p. 101).

This appears to mean that the bad biases dominating science today should be replaced by good biases inherent in reconstructed knowledge. Since the authors believe no knowledge to be unbiased or value-free, it is logical that they should urge their values as against others. They declare that "once scientists move to correct the direction of the work of other scientists by seeing the methodology of particular disciplines as class, sex, or race biased or intentionally false, it is there that reconstruction will emerge." It already has in the Soviet Union, but the authors neglect the Soviet experience, which might be regarded as the most impressive effort thus far toward new ways of knowing and reconstructive knowledge.

I find the book to have a dual agenda that could mislead or confuse naive or unperceptive readers. On the one hand it is a critique of the assumptions and values underlying a great part of present-day science and learning. I found this analysis to be the most useful and valid part of the book. On the other hand the book is advocacy for a particular point of view and while frankly stating its values and purposes, leaves the reader to infer its assumptions. The authors concede that their well-intentioned analysis must be "subject to evaluation" (p. 82).

A reasonable evaluation of chapter 3 would be that its argument is not helped by the stating of opinions as if they were demonstrable facts. For example, a note on page 84 declares without qualification that interdisciplinary work "merely brings the contradictions and flaws of any specific discipline together with other flawed disciplines, causing not a corrective, but an additional series of distortions in analysis." That the opposite or corrective effect might also be true is not acknowledged. A second example on page 83 describes environmental impact analysis as a "blind ritual" and "cosmetic step"; and note 8, which explains the author's description, indicates no knowledge whatever of how the impact statement has been implemented over the years. Their assessment is unjustifiably dated and contrary to serious empirical studies of the strengths and weaknesses of this device.

New Ways of Knowing is more significant for what it represents than for what it says. It is another of many indicators of a major shift in paradigms that characterize the present era. Much of the intellectual world today is in the predicament described decades ago by Matthew Arnold in his *Stanzas from the Grande Chartreuse:* "wandering between two worlds, one dead, the other powerless to be born." The world that Raskin and Bernstein dislike is not yet dead but is certainly undergoing transformation. As in the historical anatomy of revolutions, liberation and libertarian concepts and life-style, which reconstructive knowledge would celebrate, have characteristically been interim phenomena that gave way to new regularities when a new regime became established. Will the findings of an advancing free science (unless curbed by liberation ideology) arrive at findings that will place new ground rules and limitations on untrammeled freedom? Cigarette smokers, for example, have begun to think so.

LYNTON KEITH CALDWELL

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Citizenship and Capitalism: The Debate over Reformism. By Bryan S. Turner (London: Allen & Unwin, 1986. xii, 160p. $18.95, cloth; $8.95, paper).

Turner's essay is essentially a political reading of capitalism, starting from the assumption of the relative autonomy of the political process from the economic. Drawing heavily on the work of T. H. Marshall, Turner argues that the growth of citizenship from civil to social rights has basically transformed capitalism and that the working class (or what remains of it) has been a major beneficiary. In pursuing this line of argument he engages in a running polemic with Marx and Marxists, whom he describes as the enemies of the "reformism" he strongly advocates.

The argument proceeds through a cursory survey of ancient society and the world's major religions, followed by a critique of the writings of Marx and an exegesis of Marshall. Turner then turns to the historical processes (social struggles and social movements) that have engendered modernization and citizenship, citing class conflict, warfare, migration, and egalitarian ideologies. He concludes by examining what he describes as the paradoxical relationship between individualism and citizenship, linking the growth of social citizenship to the expansion of individualism.

This is a useful and succinct summary of the positions and arguments in favor of one strand of reformist-socialist politics. However, as an exposition and analysis of the relationship of citizenship to capitalism, it is problematic. The substantive argument for reformism lacks intellectual depth and rigor, frequently distorting the positions of Turner's adversaries and assuming what needs to be demonstrated. To begin with, the term *citizenship* is very elastic in Turner's usage. It describes at times social rights, social membership, or social participation—or all three. It is described as a mechanism for the "abatement of class struggle" and a consequence of "real struggle." It is a criterion for "universalism" and "justice" and a sphere of battle between capitalist politics and economies. *Citizenship* is loaded with so many different meanings, functions, and attributes as to cover all the vices and virtues of contemporary political discourse, so much so that even the most catholic of political confessors would have a difficult time sorting them out. The notion of *citizenship* is at times associated with vacuous terms like *social membership,* which is made compatible with a wide range of nondemocratic as well as democratic regimes. If it is made so intentionally, that compatibility is not explained.

Secondly, Turner amalgamates a variety of "reformisms" that are profoundly different.

There is the Kautskian variety (pre-World War II), which proposed reforms of capitalism leading gradually to socialism. There is the post-World War II "reformism" of German Social Democracy, which sought reforms to create a capitalist welfare state. And there is the contemporary "reformism" of Hawke, Gonzalez, and Mitterand, who seek to modernize capitalism and increase productive forces at the expense of the welfare state. Turner neither recognizes nor discusses these different reformist positions, nor does he analyze the significant shifts (the underlying reasons for the changes) or their consequences for social citizenship.

In criticizing Marx (and Marxists), Turner resorts to knocking down straw men. He accuses Marx of basing his analysis in the absolute misery of the workers ("starvation"), and of rejecting democratic reforms as "shams." As Hal Draper, among many other Marxist scholars, has pointed out, Marx fought for democratic reforms as transitional struggles toward socialism: his critique was based on the contingent and formalistic basis of democracy under capitalism. Marx's wholehearted support of the Chartist Movement and the eight-hour day are common knowledge. The real issues that Marxists raise and that reformist advocates like Turner should address are the violent constraints that capitalism has imposed on democratic institutions when workers have challenged management prerogatives. There is no discussion of the capitalist overthrow of reformist democrats in Italy in the 1920s, Germany and Spain in the 1930s, or Chile in the 1970s.

Even more surprising is Turner's failure to examine the dismissal of the democratically elected Whitlam government in Australia. Turner is correct in noting the possibility of working-class reform in some democratic capitalist settings, but he displays a singular lack of rigor in specifying the historical context, political conditions, and economic cycles in which more changes occur. Nor does he attempt to specify the limits of such reforms. He assumes a continual process of growth and improvement through struggle and democracy, with a few general caveats about the possibilities of "reversals."

Classical political theorists may also be disturbed by Turner's characterization of Plato as a "totalitarian" (p. 13)—mass party as philosopher king?—or his comparison of the Greek city-state to a modern corporation.

While the debate between reformism and revolution has been joined in this book, today what is striking is how irrelevant it is, since there are virtually no revolutionaries, and the reformists are in retreat throughout the West. The problem is not the evolution of citizenship but the demise of civility on all levels of society. Moreover, after a decade of stagnation and the collapse of the stock market, capitalism does not seem too far behind. In a word, the arguments appear somewhat anachronistic from the vantage point of the late 1980s.

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AMERICAN POLITICS

Presidential Power and Management Techniques: The Carter and Reagan Administrations in Historical Perspective. By James G. Benze, Jr. (New York: Greenwood, 1987. xii, 157p. $32.95).

It would be hard to imagine two presidencies that offer more interesting contrasts than those of Jimmy Carter and Ronald Reagan in terms of their strategies and styles in the management of public affairs. One president was compulsively interested in facts and had the intellectual capacity and stamina to absorb enormous amounts of information but was limited in his abilities to communicate the broad themes of his administration; the other is largely indifferent to the day-to-day details of his presidency and apparently ignorant about important actions taken by his foreign policy advisors but is unusually skilled in the arts of political communication. One was elected after promising to make federal programs more effective and to give the United States a government as good as its people; the other was elected on a pledge to get government off our backs and to dismantle much of the federal colossus. One took on a very large agenda of domestic and foreign policy initiatives with some successes and failures across the board; the other took on a narrower set of issues with better results in achieving a limited list of objectives. One lost his bid for reelection; the other won a large national majority and a second term.

The material taken up by James Benze has tremendous potential, but unfortunately, in Benze's treatment of that material much of that potential is missed. The first three chapters of Benze's book summarize the efforts of postwar presidents to implement their decisions and policies and provide a brief history of the principal management techniques used to accomplish these purposes—budgeting, reorganization, and the manipulation of personnel. These chapters are reasonably well done but contain little that would not already be familiar to students of the presidency and public administration. The chapters on Carter and Reagan are too brief and give us only a sketchy account of how specific management techniques were used in these recent administrations. Finally, the book contains several chapters that analyze the results of surveys conducted by the author in 1980 and 1984, when questionnaires were sent to senior federal administrators (mostly bureaucrats but also some political appointees) asking for their evaluations of presidential management in the preceding four years.

The decision to survey bureaucrats in order to evaluate presidential management makes it difficult to say very much about whether or not Carter and Reagan were successful in getting their policies implemented. Benze offers no objective measure of policy implementation or compliance and the impressions of federal officials on these issues constitute a poor substitute. It is a bit like rating a chef by interviewing the waiters. The survey results do tell us something about how senior bureaucrats have viewed our recent chief executives, and these results may well be of interest to students of public administration and U.S. bureaucratic culture. The portraits of Carter and Reagan that emerge from the surveys roughly conform to prevailing media portraits of the two presidents. Carter is described as a failed chief executive who was intelligent and honest but unable to perform as an effective leader; and Reagan is described as a successful president whose political skills compensated for his much more detached approach to policy making.

In his conclusion Benze asserts that successful presidential management depends on both the technique he has described in his book and on the skillful use of political power. This is no doubt true, but it gives such a broad interpretation to what is included as part of presidential management that it becomes difficult to imagine anything that a president might do which would not be part of a management strategy. And if management has this expansive definition, it is hard to understand why Benze wishes to draw our attention to budgeting, reorganization, and personnel.

James Benze raises fundamental issues about how presidents, and particularly our two most recent presidents, can and should deal with the government they ostensibly run—questions that have received better treatment in Colin Campbell's new book, *Managing the Presidency*. In the end he offers few original or lasting answers to these important questions.

ROBERT A. STRONG

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Disabled Policy: America's Programs for the Handicapped. By Edward D. Berkowitz (New York: Cambridge University Press, 1987. xiii, 280p. $27.50).

As researchers begin to discover the vast social and political problems created by disability, a spate of books has appeared with titles using *disabled* as a modifier to suggest the feebleness of prior attempts to cope with these issues of public policy and human rights by various aspects of the political system. Berkowitz's study is clearly one of the best of these. This comprehensive review traces the evolution of legislation about disability from worker's compensation in the Progressive era to the formulation of the Social Security Disability Insurance program as well as vocational rehabilitation to the contemporary independent living and disability rights movement. Berkowitz not only documents the inadequacy of earlier policies but also provides a valuable introduction appropriate for anyone seeking to become familiar with this important field.

Essentially, however, Berkowitz's conclusion is the same as his opening statement: "America has no disability policy." He laments the fact that disability programs are used primarily as a means of withdrawing workers from the labor force rather than as a method of enabling them to find a productive role in society, but he has no dramatic alternative to propose. Operating almost exclusively within the conventional paradigm that defines disability as an inability or limitation on the amount or kind of work that a person can perform, Berkowitz is unable to find a satisfactory answer to the questions that he raises. While there is obviously great value to be derived from insightful criticism of the present chaotic state of disability policy, a resolution of these problems would of course be even more valuable.

Part of the difficulty might be ascribed to differences in disciplinary orientations. As a historian, Berkowitz believes that history "brings order to the labyrinth of disability programs." He asserts that historians understand, as other policy analysts might not, that the present follows logically from the past (p. 228). With these insights, a historian can open up a clear view of the larger picture that is denied by the structure of modern policy-making. Yet the perspective of history also involves the danger of becoming mired in prior definitions and paradigms rather than accepting new approaches to policy problems.

Ironically, the major strength of Berkowitz's work also reveals its principal weakness. Unlike many other researchers, Berkowitz devotes significant attention to the political changes currently being sought by the disability rights movement. This chapter is the most valuable feature of the book. But in the policy recommendations that he commendably offers in his conclusion, he fails to accept the view that discrimination is the primary problem facing citizens with disabilities—he proposes the enforcement of civil rights laws, along with the prevention of disability, almost as an afterthought. Hence his reforms amount to minor tinkering, and the basic inconsistency encompassed by existing disability policy is never fully addressed.

This position poses an additional dilemma for policy analysts who wish to move beyond historical background to evaluate alternative remedies. Some political scientists, such as Aaron Wildavsky, believe that disability policy, like many issues, can be studied objectively. This view, however, has been criticized for ignoring the fact that people—in fact, an oppressed minority—comprise the object of such programs. Others, pointing to the significant contributions made by black, Hispanic, and feminist scholars, contend that the value of such research can be enhanced by infusing it with perspectives derived from intimate experience with a disability. The analysis presented in *Disabled Policy* seems to imply the impossibility of resolving this quandary.

As Berkowitz perceptively points out, there appear to be vast differences between the policy prescriptions appropriate to the needs of a middle-aged laborer who may feel it is too late to benefit from retraining, and to the desires of prominent leaders of the disability rights movement such as Judy Heumann, Ed Roberts, and Lex Frieden, who acquired their disabilities as young people. Yet this paradox may point the way to a solution to the fundamental problem of the politics of disability. There seems to be no similarity between the anonymous laborer who wants to leave the work force and these leaders, who are seeking active or productive lives for all disabled persons, because there is presently no basis for a shared sense of political identity. This laborer, like millions of disabled citizens, appears stymied by the prejudice evoked by disability, which is also the primary source of oppression. From this perspective, therefore, the principal remedy for the social and political problems produced by disability might be found in the development of a positive common identity that could be used as a basis for organizing a powerful electoral constituency of disabled voters, rather than in the reforms recommended by experts and professionals.

Although efforts to develop a positive sense of political identity have not yet been successful in securing a consensus among disabled citizens, there seems to be little doubt that these strivings will affect both the substance and the style of future research on disability policy. While seemingly a minor quibble about Berkowitz's highly readable book, this process might appropriately encourage the use of *disabled* solely as an adjective, never as a noun, the latter implying that a person's entire self-identity is subsumed by a physical impairment. Like other minorities, people with disabilities are seeking the right to strike down the stereotypes that have oppressed them in the past and to be called by names they choose for themselves. According to a recent Harris survey, a major proportion of disabled citizens consider themselves to be members of a minority group; and many are beginning to recognize that their principal problems stem from discriminatory attitudes and behavior rather than from their own functional limitations. There seem to be compelling reasons, therefore, to make scrupulous efforts to avoid the subtle implication that a feeble policy is a natural or appropriate reflection of a supposedly feeble minority.

HARLAN HAHN

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Pesticides and Politics: The Life Cycle of a Public Issue. By Christopher J. Bosso (Pittsburgh: University of Pittsburgh Press, 1987. xvi, 294p. $29.95).

Christopher Bosso's book is a welcome addition to the policy studies literature on two counts: it covers an area that has been relatively neglected in environmental policy analysis, and it makes an important contribution to the wider debate over how we arrived at the current state of policy gridlock in Washington. Bosso traces the course of federal pesticides policy from the passage of the Federal Insecticide, Fungicide, and Rodenticide Act (FIFRA) of 1947 through the great gypsy moth and fire ant eradication campaigns of the 1950s and 1960s, the environmental reform movement culminating in revision of FIFRA in 1972, the banning of DDT and several other pesticides in the 1970s, and the subsequent deadlock over implementation and further revision of the law in the 1980s. On the whole it is a sad story, not only because pesticide control has yet to make a serious dent in the chemical dependency of U.S. agriculture but because of what it shows about the difficulties of achieving *any* policy closure against entrenched interest group opposition in our political system.

That is not to say that this is just another case study in interest group politics. Bosso takes a much more comprehensive approach, introducing numerous policy models and drawing on many of the richest veins in political science, particularly the work of E. E. Schattschneider and Charles O. Jones, one of his mentors. As a longitudinal policy analysis, the book perhaps most resembles Jones's *Clean Air,* though it has the advantage of longer perspective on problems of implementation than did Jones's book on the Clean Air Act circa 1974. Many of Bosso's conclusions about "policy failure" are also similar to Jones's: the requirements of the 1972 Federal Environmental Pesticide Control Act (FEPCA), which revised FIFRA and transferred responsibility for pesticide registration from the Department of Agriculture to the Environmental Protection Agency (EPA) were far beyond the technical and budgetary capabilities of EPA to implement and invited endless litigation and obstruction. However, Bosso places these shortcomings in the larger context of the changing *structure* of policy-making, particularly in Congress. Drawing here on Schattschneider's fundamental observations that structure biases policy outcomes, and that the scope of group conflict and management of public participation are key variables in the process, he is able to show that pesticides policy-making has moved through three stages: from (1) firm control by a closed "subgovernment" (dominated for many years by Representative Jamie Whitten of Mississippi, who chaired the House Appropriations Subcommittee on Agriculture), through (2) a phase of "pluralist competition," in which participation was expanded to include environmentalists and public health advocates, to (3) its current state of "presence politics," in which policy authority is thoroughly fragmented in Congress and the policy network (Heclo) has been extended to incorporate policy "experts" on all sides who skirmish at the margins but make little real progress toward policy resolution.

Bosso's book summarizes much of recent thinking in policy studies; indeed, if there is a criticism to be made of his book, it is perhaps that it is overburdened with concepts and models to the point where one loses touch with the main thrust of the analysis. There is also not quite enough focus on the substantive problem, namely the costs and benefits of pesticides. Although Bosso is correct in arguing that there is a vast gray area of uncertainty about the environmental and health effects of pesticides, the book would have been stronger had he made more of an effort to review the evidence. Nor, despite the attention he gives to the "pesticides subgovernment," does he identify or discuss the corporate interests involved in chemical agriculture. Finally, although many of the points Bosso makes about the failures of pesticide regulation are valid in other areas of environmental policy, his analysis should not be generalized to all policy in this field. Many pieces of environmental legislation reflected a basic shift in public opinion (not simply a Downsian attention cycle or spate of "pluralist competition") that has proven durable and supportive of genuine environmental improvement. Nevertheless, Bosso's sobering warnings of policy stasis are well taken.

NORMAN J. VIG

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The White House and Capitol Hill: The Politics of Persuasion. By Nigel Bowles (New York: Oxford University Press, 1987. ix, 261p. $39.95).

This work focuses on presidential-congressional relations. It begins with a description of the context and organization of the Johnson White House's Office of Congressional Relations (OCR), including the development of the office, presidential involvement in congressional liaison, the nature of the OCR staff, the handling of congressional correspondence, the OCR's relations with departmental liaison offices and congressional leaders, the role of non-OCR White House staff, head counting, and congressional committee politics and assignments.

This discussion is followed by one chapter on projects and patronage and another on the role of small favors and lobbying. The latter include gifts, meetings with the president, photos, trips on the presidential yacht or Air Force One, congressional receptions at the White House, bill-signing ceremonies, election assistance, and personal lobbying. The author describes the Johnson White House's use of these potential resources, and concludes that they are more useful in creating a favorable political climate for presidential influence in the long run than in obtaining short-term support on a specific bill.

The author then presents two chapter-length case studies of executive-legislative relations from the Johnson administration. One focuses on the passage of the income tax surcharge of 1968, while the second examines the nominations of Abe Fortas as chief justice and Homer Thornberry as associate justice of the Supreme Court in the same year. In both studies the author reveals the weakness of the OCR in Johnson's last years in office. Finally, there are chapters that provide broad overviews of the congressional relations of Jimmy Carter and Ronald Reagan.

Most of this is well-trodden ground, but the author adds some interesting bits and pieces to the portrait of Lyndon Johnson's relations with Congress. Since they have become part of U.S. political folklore, the author's critical correctives to the legends that have accumulated around the Johnson White House are particularly useful.

Nevertheless, readers would be better served if the author had integrated his work with those of others who have done work on White House legislative liaison. There is now substantial literature on the subject, and the author largely ignores it. As a result, opportunities to build on previous scholarship are lost.

The presentation of the small picture is generally well done. The author often shows considerable astuteness in his discussion of discrete aspects of presidential-congressional relations. He does not accept things at face value, and his discussion is not constrained by a rigid perspective.

The big picture is less satisfactory, however. There is no theoretical underpinning to the study, no questions to guide an analysis. Thus the descriptions of White House behavior do not lead us to generalizations in which we can have confidence.

Essentially, the author begins with the assumption that the White House legislative liaison operation is important in influencing members of Congress to support the president and therefore begs the essential question of just what contribution it can make to presidential leadership. Much of the material in the book actually belies this premise. For example, the author concludes both case studies with the view that circumstances made it unlikely that the president could have done any better than he did, even if the OCR had displayed more skill.

The author concludes that "this book has shown that . . . the skill with which presidents orchestrate their relations with Congress through the OCR . . . materially affects the fate of their legislative proposals" (p. 247). What it actually does is provide additional evidence that the contribution of presidential legislative skills is at the margins rather than the core of presidential leadership of Congress.

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All Organizations Are Public: Bridging Public and Private Organizational Theories. By Barry Bozeman (San Francisco: Jossey-Bass, 1987. xi, 184p. $21.95).

Do public organizations differ significantly from private organizations? If so, how do those differences affect managerial effectiveness? That is what Barry Bozeman calls the "publicness puzzle." His main purpose is to show that the public-private dichotomy is more imagined than real and that all organizations are public. It is a conclusion that seems radical at first thought, but on more reflection is common sense. It is a corollary to the conclusion that every economy is a political economy. All economies are political because they operate under rules and procedures established by political authority. Bozeman argues that "all organizations are public because political authority affects some of the behavior and processes of all organizations" (p. 83).

Two main authority types—economic and political—are identified and discussed at some length. Economic authority is explained in the context of property rights theory. The concepts of private and public goods are discussed. The importance of residual claimants is recognized. Linking authority to act with responsibility for consequences is acknowledged as an important contribution of private property rights. Bozeman suggests that property rights theory is useful because "a wide variety of organizations, including public administrations, can possess property rights or their equivalent" and that "just as publicness has pervasive effects on business, 'privateness' (conveniently measured by constructs derived from property rights theory) has pervasive effects on government and other nonprofit organizations" (p. 59).

Political authority is more difficult to define than economic authority, but Bozeman does an admirable job by identifying different levels of political authority, combining them into a "triadic model of the effects of political authority on organization behavior" (p. 79), and showing how the model is applicable to business, third-sector, government enterprise, mixed form organizations, and traditional government agencies. He concludes, "The organization subject to political authority may, as a result, become more or less powerful, more or less effective, and/or more or less adaptive, but it is sure to be changed" (p. 82).

Bozeman suggests we might better understand and evaluate the management of public and private institutions if we thought of them as having a mix of economic and political authority. That mix is described in the Cartesian plane defined by economic authority on an x-axis and political authority on a y-axis. When various organizations are arrayed in the space it is possible to recognize that economic authority and political authority are sometimes complementary and sometimes countervailing, depending on the nature and purpose of the organization. Although Bozeman does not explore it, positing this Cartesian plane suggests that economic entrepreneurs should scan the private *and* public horizons when looking for new opportunities or better tools for thwarting competitors.

A problem with the book is that it only begins to unravel the "publicness puzzle." But Bozeman recognizes that and proposes how his model might be applied to management research, education, and application. His "most fundamental lesson" is straightforward and unequivocal: "Public management is too confining. What is needed is managerial theory and prescription for managing publicness."

The book is a significant contribution to understanding public and private organizational theories. Just as political economy provides greater understanding into the processes and outcomes of private and public sectors, the concepts proposed by Bozeman will help us to understand the operation of public and private institutions better. As we learn to apply these concepts, we may even avoid falling into the trap of assuming institutions are either public or private and think of them as either "more or less public."

RANDY T. SIMMONS

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Leadership and Innovation: A Biographical Perspective on Entrepreneurs in Government. Edited by Jameson W. Doig and Erwin C. Hargrove with a foreword by Richard E. Neustadt (Baltimore: Johns Hopkins University Press, 1987. xii, 459p. $39.50).

Eugene Lewis's 1980 book, *Public Entrepreneurship: Toward a Theory of Bureaucratic Political Power*, was innovative in at least two respects. First, it was at the time one of the few works in U.S. political science to point to the fact that individuals sometimes do make a difference. Second, it demonstrated in no uncertain terms that such individuals—known in common parlance as leaders—can be found in the unlikeliest of places, that is, in public organizations. We were taught, in short, that political power and influence are exerted not only by elected officials but also by "public entrepreneurs" who manage, in large part by force of personality, to "alter greatly the existing pattern of allocation of scarce public resources" (p. 9).

Doig and Hargrove's collection of essays on "entrepreneurs in government" is an offspring of Lewis's pathbreaking volume. They, too, were interested in individuals who held high-level executive positions during a time when their agencies changed course and who appear to have played major roles both in devising and implementing these significant initiatives. This book then includes chapters on twelve government officials who fit the above criteria and one on a private executive whose terrain is similar to his counterparts in the public sector.

Doig and Hargrove coauthor an introductory chapter that sets the stage for the biographical essays to follow. They make the point that while their own emphasis is on the impact of individuals, context is probably equally important. In particular they cite three external factors that "provide fertile ground for leadership." They are a governmental system characterized by fragmentation and overlap and therefore relatively hospitable to experimentation; a public voice that is capable of making itself heard on behalf of new social values; and new technologies that provide visionary government officials with heretofore unimagined opportunities.

The editors also cite three personal characteristics that "may be crucial" to successful leadership: the capacity to engage in rational analysis, the ability to see new possibilities, and the desire to make a difference. And they touch on an original theme when they point out that almost all of their thirteen subjects experienced "significant reverses" after their striking success. "How," Doig and Hargrove ask, "does one account for failure after success has been achieved?" (Their tentative answer: a shift of political support away from the entrepreneur.)

I dwell on the editors' all-too-brief opening essay because thereafter readers are on their own; that is, it is left up to us to extract the similarities and differences in how leadership was exercised and to draw our own conclusions about the implications of these for others in similar positions. It would have been helpful if the editors had drafted a concluding essay to sort and comment on the mass of biographical data their book provides. Such an ordering would, in and of itself, have constituted a theoretical contribution.

Most of the essays are not, in fact, "biographical perspectives," for they do no more than touch on life history. Rather—and this is in line with the editors' instructions to the contributors (p. 8)—they are chronicles of the professional initiatives for which the subjects of this book were best known. In keeping with the two clusters of skills that the editors identify as being especially important—the adroit use of rhetoric and coalition-building skills—the book is divided into two parts. Part 1, on rhetorical leaders, features chapters on David Lilienthal, Gifford Pinchot, Hyman Rickover (written by Eugene Lewis and based on *Public Entrepreneurship)*, Austin Tobin, and James Webb. Part 2, on administrative entrepreneurs, has essays on Nancy Hanks, Wilbur Cohen and Robert Ball (covered together in a strong chapter by Theodore Marmor), Elmer Staats, Marriner Eccles, Bernard O'Keefe, James Forrestal, and Robert McNamara.

This, then, is primarily a collection of stories about organization men, and one woman, who successfully exerted power, authority, and influence to bring about change. Their tales are in the main well told and they provide further evidence—should any still be needed—of the impact of key individuals on political life. As Doig and Hargrove point out, such documentation is not only an intellectual corrective to a past bias in U.S. political science; as well, it has the salutory effect of conveying to our best potential leaders that the private sector is not the only outlet for their energies.

BARBARA KELLERMAN

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The New American State: Bureaucracies and Policies since World War II. Edited by Louis Galambos (Baltimore: Johns Hopkins University Press, 1987. 227p. $27.50, cloth; $11.95, paper).

Crisis and Leviathan: Critical Episodes in the Growth of American Government. By Robert Higgs (New York: Oxford University Press, 1987. xix, 350p. $24.95, cloth).

In a slim but provocative volume Louis Galambos has assembled a diverse and spirited collection of essays on the "new American state." In separate chapters on environmental policy, social security policy, national security policy, and macroeconomic policy, four social scientists ask who really runs the modern state. An introduction by the editor and a conclusion by Matthew Crenson and Francis Rourke help to integrate this inquiry without fabricating an artificial consensus.

All the authors agree that we have an administrative state and that it is here to stay. They also agree that the federal bureaucracy has been challenged by politicians, citizens, or both. Samuel Hays, in his essay on environmental policy, stresses control from below, with strong citizens groups articulating the concerns of a consumer-minded middle class. Heywood Fleisig, in his essay on macroeconomic policy, stresses control from above, with presidents and legislators exerting leverage over fiscal and monetary policy. Crenson and Rourke, who discuss both foreign and domestic agencies, see evidence of control from above and below.

While the authors agree that the bureaucracy is under siege, they disagree on the bureaucracy's response. Charles Neu, in a chapter on national security policy, argues that no recent president has succeeded in harnessing the national security bureaucracy. Similarly, Carolyn Weaver concludes that the Social Security Administration has managed to defeat most efforts to limit benefits or introduce private competition. On the other hand, Crenson and Rourke perceive a resurgence of political control since World War II, and Fleisig doubts that politicians have ever truly relinquished control over macroeconomic policy. Hays sees a strong bureaucracy but also a permeable one. Thus when environmental agencies fail to deliver environmental benefits, the middle class becomes aroused and insists on reform.

Although the central focus of the book is who runs the state, the authors offer many valuable insights into how the state is run. A recurring theme is the central importance of information—computer models, environmental impact statements, cost-benefit analysis, and so on. For example, the Social Security Administration has used a data monopoly and sophisticated computer models to deflect external challenges. Conversely, regulated industries and environmental groups have employed research to challenge administrative agency decisions. Clearly, information is a double-edged sword in the new administrative state.

Despite its considerable strengths, this book has two weaknesses. First, neither the introduction nor the conclusion develops an analytical framework that might explain why some agencies are highly permeable while others are not. A model based on issue characteristics, bureaucratic characteristics, and constituency characteristics would have been a welcome addition. Second, the authors sometimes treat political control in gross terms, failing to specify subtle but important differences that flow from legislative oversight as opposed to statutory controls, public hearings as opposed to proxy advocacy, personnel reforms as opposed to executive orders. Ultimately, we learn more about how control is exercised in the new state than about the consequences of exercising control in different ways. Still, we learn a great deal, and the chapters are well written. The Galambos book is likely to be well received in both public administration and U.S. history courses.

If Galambos and his collaborators have learned to live with the administrative state, Robert Higgs has not. In *Crisis and Leviathan,* he analyzes and laments the growth of "big government" in the twentieth century. He explains that growth by focusing on major crises and the ideologies they generate. The cumulative legacy of these crises, he argues, is a "cost-imposing, cost-concealing" command economy.

Crisis and Leviathan will strike many readers as a throwback to the "great events" school of history that preceded the "social history" approach with which Samuel Hays is closely identified. From Higgs's perspective, mass values simply mirror elite values, which in turn are shaped by critical episodes, such as wars and depressions. Higgs would seem to agree with Emerson that "events are in the saddle and ride mankind."

In one respect, Higgs's book is an improvement over recent empirical studies of government growth. He does not confine his attention to increases in government spending or employment. Rather, he examines diverse forms of government coercion, including conscription, wage and price controls, and the seizure of private property. He also looks beyond legislative appropriations to executive orders, administrative agency decisions, and Supreme Court rulings.

Unfortunately, however, Higgs focuses disproportionately on two kinds of crises: wars and depressions. Consequently, he neglects significant accretions to the administrative state that are not attributable to either: for example, the Administrative Procedure Act of 1946, the War on Poverty, and the new social regulation of the 1970s. He also has trouble accounting for deregulation of the airline industry, the banking industry, the transportation industry, and the communications industry, all of which are inconsistent with the "dominant" ideology of a command economy.

Stylistically, the book is disconcerting. It is repetitive and tendentious. It contains too many quotes, too many definitions, and too many sweeping assertions. It is often glib in its treatment of both politicians and social scientists. For example, Higgs dismisses Progressives and New Dealers as closet socialists. Similarly, he derides the "quasi-statistical" research of behavioral scientists, whose thinking he finds "simple-minded." In fact, behavioral scientists are not the only observers to attribute considerable historical significance to social movements, urbanization, and demographic changes. By focusing on a limited number of key events, Higgs has written a history that is old-fashioned and incomplete.

WILLIAM T. GORMLEY, JR.

University of Wisconsin, Madison

The Sound of Leadership: Presidential Communication in the Modern Age. By Roderick P. Hart (Chicago: University of Chicago Press, 1987. xxii, 277p. $39.95, cloth; $14.95, paper).

This work makes an important contribution to the recent group of studies exploring the sources, patterns, and consequences of presidential rhetoric. The analysis is based upon a coding of the nearly 10 thousand speaking roles engaged in by presidents between 1945 and 1985 as recorded in the *Public Papers of the Presidents.* A comprehensive coding scheme allows the author to make extensive comparisons regarding changes since 1945 on such factors as types of audiences addressed, location of speeches, primary topics, and timing within the reelection cycle. In addition, the work includes a comparison of network responses to a group of major addresses between 1969 and 1978, and frequent quotations from speeches for illustration and analysis.

The book is particularly effective in providing a clear documentation of the dramatic changes that have occurred in presidential speaking roles since 1945. Presidents increasingly (1) engage in ceremonial activities, (2) undertake international addresses and speeches outside Washington, and (3) conduct during the first three years speaking schedules that resemble those undertaken during the reelection year. Overall, the changes show a dramatic increase in the total amount of activity. Presidents Kennedy, Johnson, and Nixon all substantially increased the levels attained by Truman and Eisenhower, while Ford, Carter, and Reagan sharply outdistanced their predecessors. Irrespective of partisan differences, recent presidents have been devoting much more time to their speaking roles.

Readers will find useful insights into individual presidents in the interpretations of basic data. Kennedy is seen as having fundamentally increased the media sensitivity of modern presidents. Johnson was uniquely a home state speaker, seemingly in search of more hospitable audiences. For Nixon, international ceremonies held an unusually strong attraction. Ford was a uniquely frequent speaker, particularly in 1976—with speeches occurring on the average of once every six hours. In efforts to use speaking to improve his popularity, Carter was singularly ineffective. For Reagan, the continuation of high levels of speaking occurred along with a greater tendency to speak in controlled settings to reduce potentials for difficult questions. By carefully reviewing longitudinal data, the author provides an important example of the manner in which the study of distinctiveness and trends in presidential behavior can be usefully integrated.

As an interpretation of the consequences of changes in presidential uses of speaking, the book is less complete. Perhaps in part because of the desire to explore a large data set, the author confines the analysis of consequences largely to implications that can be drawn from that basic data. In those analyses, some readers will be disappointed with the lack of more precise modeling of explanatory relationships. There is also little attention in those discussions to studies that have examined public opinion changes on specific issues, or the consequences of presidential speaking on legislative behavior. Similarly, while the location of presidential speaking during election years is carefully documented, that discussion is not integrated with studies that have looked at elections as coalition-building efforts. To reach conclusions about consequences more convincingly, studies of presidential speaking will ultimately require greater integration of findings in related areas of specialization than is present in this volume.

The author's conclusions are nonetheless well worth careful reflection in efforts to understand the major changes that are clearly taking place in presidential politics. Those views are presented in the context of considerable concern with the emergence of a media-oriented presidency and include the following: (1) presidents speak more, but do not educate, (2) presidents increasingly plan their activities simply to generate publicity, (3) there is an increasing tendency for presidents to define their role as simply giving a speech rather than actually developing policies, and (4) Reagan's leadership style represents a logical outcome of changes toward a media-oriented presidency. Provocative interpretations plus useful basic data make this book a valuable contribution.

William W. Lammers

University of Southern California

The Life of the Party: Democratic Prospects in 1988 and Beyond. By Robert Kuttner (New York: Viking, 1987. xi, 265p. $18.95).

Robert Kuttner, an economics and politics writer for the *New Republic*, the *Boston Globe*, and *Business Week* offers a provocative populist treatise suggesting how the Democratic party might reclaim its historic soul and its electoral majority. His basic theme, diagnosis, and message is that Democrats, especially of the Jimmy Carter and neoliberal mold, have lost sight of their traditional strengths and should return to being the party of economic populism and the wage-earning citizen.

Kuttner rejects the notion that to win the White House and similar political prizes the Democrats have to move to the center and imitate Reaganism. He also rejects the idea that citizens are apathetic or that they are turning against the New Deal programs. He acknowledges a change in demographics, a decline in coalition politics, and the identity crisis Democrats have undergone as a result of Vietnam and Reagan's popularity.

Like many analysts, Kuttner describes how Democrats have fallen behind the Republicans in fund-raising and the technological tools of modern day campaigns. However, he faults the Robert Strauss (former Democratic National Committee chairman) approach of trying to outdo Republicans at their own game by courting business Political Action Committee (PAC) money, and suggests instead that they seek larger numbers of small contributions and lessen their dependency on big business and PACs in general.

He decries the decline of parties and, like V. O. Key and others, notes that a regime without parties is necessarily a conservative regime. Kuttner pays too little attention to the fact that the decomposition of parties has been taking place for a very long time. It is not just that the Democrats are badly split or themeless or that the Republicans are better at raising money and using polls. The Progressives mounted a purposeful and in some ways successful campaign to undermine parties in the early part of the century.

Kuttner is especially critical of the neoliberals. He sees them as mildly helpful in diagnosis yet virtually useless as strategists for the future. What does Kuttner propose? His prescription is a left-wing "back-to-the-future" course. He would have the Democrats return to their successes of the 1930s and 1940s as a means for winning both blue-collar and new-collar votes. Issues of social and foreign policy disrupt the party. Economic populism unites Democrats and their natural constituency. Only a broad economic program that serves the workers and the little guy makes sense. The Democratic party, he insists, must be the party of opportunity, inclusion, and the affirmative use of government to temper the extremes of the capitalist market system.

Thus he promotes national health insurance, accelerated investments in public education, comprehensive child care policies and Paul Simon's WPA-style measure to put unemployed people back to work rebuilding roads, bridges, parks, and other public facilities. He also points to Jim Hightower, the elected state agriculture commissioner in Texas, as a positive example of economic populism combined with culturally mainstream politics. In short, if Democrats are going to win, it will be because they affirm that government is not part of the problem but rather—in the Wilson, FDR, Truman and JFK tradition—part of the answer for those who live from paycheck to paycheck.

Kuttner writes well and borrows liberally from political scientists and economists. He is a decided partisan. Doubtless he is correct that economic issues tend to unify Democrats, while social and foreign policy issues have fractured the party in the past generation. He also points to good old traditional U.S. interest politics: provide the voters what they need.

However, he glosses over how much economic and foreign policy are increasingly and irreversibly intertwined. He also ignores rather completely the various cycle or pendulum theories provocatively put forward by James Sundquist, Albert Hirschman, Arthur M. Schlesinger, Jr., and James David Barber, among others. Kuttner inadequately addresses how—even if Democrats emphasize economic populism—their disputes over foreign and defense policy might be resolved. Kuttner favors tax increases and, at least implicitly, would cut defense spending. How would he retain the Sam Nunn wing of the party? Defense cutbacks put people out of work along Route 128, in South Carolina, Colorado, Texas, and southern California. Kuttner probably has ideas for employing these folks in other manufacturing, research, and service industries, but his program is fuzzy on this subject. If the Democrats have reaped political dividends from the rise of the welfare state in the United States—albeit a conservative welfare state—so also the Republicans have reaped dividends from the rise of the warfare state. Kuttner pays this less attention than it deserves.

He is convinced, as most political scientists are, that Reagan's 1984 victory was due to the widespread belief that the economy was doing well, and far less attributable to Reagan's repetitious government-bashing rhetoric. Kuttner is at his best when he calls on Democrats not to try to mimic the Republicans, but to build their own party, develop their own strategies, reclaim those FDR and Great Society roots, and thereby become a distinct and successful programmatic party that serves its true constituencies.

Kuttner is a bit less convincing that this will work in the suburbs, the Southwest, the Rockies, or the South. Boston-based Kuttner, who occasionally lectures at the JFK School of Government, may have a Massachusetts lens through which he views his subject. He may also underestimate the depth of the antigovernment, antiwelfare, and antiregulation mood that characterizes middle-class voters.

Still, his book is a welcome counterpoint to the now tiresome Reagan rhetoric, to the ceaseless pounding affirmative government takes from both the neoliberals and neoconservatives and from the majority of the so-called think tanks in the U.S., which regularly trivialize government problem solving by dwelling on failed policies or unanticipated consequences of otherwise effective federal programs.

THOMAS E. CRONIN

Colorado College

The New Black Politics: The Search for Political Power. 2d ed. Edited by Michael Preston, Lenneal Henderson, Jr., and Paul Puryear (New York: Longman, 1987. x, 293p. $29.95, cloth; $12.95, paper).

This is the second edition of a fine and widely used text. It is a mark of its distinction, in fact, that it has already gone into the second edition. As fine as the first volume was, the second is not just a reprint. The readings here are mostly either new or substantially revised, building on the ever-changing circumstances in this field. This is an excellent book that thrives as much on the independent strength of the various pieces as on the efforts of the editors to achieve unity of theme. The rich array of data, subtlety of analysis, creative presentation, and range of subjects offered are outstanding. These (mostly mature) scholars know their subjects intimately and bring a wealth of analytical skill to their tasks. This edition will be as deservedly popular as the last, and there is already reason to await a third edition.

The text focuses on the "new black politics," which could be construed to refer to post-1968 electoral politics. Obviously, much more than that is involved. But consider that such a volume, documenting almost entirely cases of black electoral politics, could hardly have been written 25 years ago. The phenomena these scholars describe are, from that standpoint, "new." Consequently, the themes that dominate the analyses are particular to the electoral arena: mobilization of black constituencies via registration and voting, strategies for institutionalizing social change ideologies, censuses of electoral success, short-term public policy outputs, and revisionist theories about the fit of black political analyses with general U.S. political analyses. And yet perhaps the most common (and quintessentially familiar) theme is the importance of racial polarization in biracial political competition.

There are but three parts to the book, although there are any number of other ways the editors might have divided the material and any number of other subjects they might have considered. Despite this, what is offered has breadth and the three parts are in any case less discrete than they may seem.

Part 1 deals with national and state politics and includes pieces on presidential, congressional, and gubernatorial elections. (A piece on the courts is the sole one not dealing with actual election circumstances in the entire book.) Lenneal Henderson surveys black participation in presidential elections, with an emphasis on the post-1960 period. He makes some significant points about the consequences various political environmental conditions may have for black influences on such elections and the independent importance of "black political values" upon any presidential elections. Lucius Barker shows the vital importance of the Jesse Jackson campaign as a tool for bargaining for political goods and as a symbol for inciting and intensifying black mobilization. By this view Jackson is seen as the best model to do for black voters what other candidates do for citizens in the general electorate.

Two articles deal with electoral defeats. Mary Coleman and Leslie McLemore offer up a subtle analysis of failure in a congressional district in the Mississippi delta. While brief—one wishes for more—the article is solid and raises some tantalizing issues about candidate selection, the conventional wisdom of the impact of socio-economic status (SES) on black behavior, and the independence of black electors even in majority districts. It is interesting how these authors' findings for rural Mississippi compare and contrast with those of Preston on urban Chicago later on in the volume. Moreover, one awaits the Coleman and McLemore analysis of the 1986 election in this district, where a black candidate did win. No less successful in this section is Charles Henry on the defeat of Mayor Bradley for governor of California. The author moves through a mass of election return data to show convincingly that race was a significant factor in Bradley's loss. Racially polarized voting here, as in Mississippi, seemed to turn back a candidate representing centrist values that are usually admired in the general population. Moreover, the candidate also sought to diminish his racial background and sensibilities as factors in the election.

In part 2 Linda Williams does an excellent historical survey of who votes, who is elected, and to what offices. This analysis shows the great range—albeit not a representative one—of offices held by blacks in this new era. Organizationally her piece might best have preceded the others.

The final section is on urban politics and public policy, and considers only big city mayors. Good pieces are offered on Detroit, New Orleans, and Philadelphia by Rich, Perry and Stokes, and Ransom, respectively. Michael Preston takes up the analysis of Chicago and as usual does it with consummate skill. He works from a body of theory in the general literature but uses it in a novel way to elaborate the particular circumstances of the black citizen mobilization, personal leadership, and ultimate success in the city. The author provides some interesting and confounding twists on the SES model, which seems to work in Chicago sometimes but not in other cities. The article is accompanied by masses of excellent data, though it is a relatively short piece. William Nelson—also as usual—returns to Cleveland with profound new insights. The analysis goes further than others herein in the comparative vein because of the longer experience in Cleveland. Nelson compares Stokes's relative success in meeting the challenge in serving the black electorate, to the lack of success of the succeeding regime. Indeed the latter may be merely overseeing the demobilization of the black insurgent forces in Cleveland, according to the author. Two of the strongest analytical points here are regarding the need for independent black organization and for some consistent means for maintaining organizational integrity for the sponsoring constituency. All told, these analyses of mayors provide good range for useful comparisons and contrasts.

MINION K. C. MORRISON

Syracuse University

Breaking the Rules: Bureaucracy and Reform in Public Housing. By Jon Pynoos (New York: Plenum, 1986. xiv, 221p. $29.50).

Jon Pynoos's *Breaking the Rules* is a detailed attempt at examining the nature of bureaucratic discretion. Pynoos examines how applicants are assigned to vacancies at the Boston Housing Authority. He wishes to delimit the circumstances under which selection rules are broken and why attempts at reform succeed or fail. Although discretion generally resides among the top echelons in bureaucracy, scholars such as Dahl, Lipsky, and Prottas have clearly demonstrated that street-level bureaucrats make many independent decisions in their daily contact with the public, decisions that can affect the well-being of citizens. Pynoos's effort is an extension of the street-level literature. He is able to look at discretion over time and under changing conditions.

In fact, Pynoos finds himself addressing a conundrum. Various writers on bureaucracy have noted that when goals are absent or vague, rules can become the goals for employees. Yet at the Boston Housing Authority, goals were vague but rules did not become goals; rather, rules were circumvented or ignored. Pynoos's detailed examination of tenant selection practices over a decade clearly illustrates the failure to follow specified rules.

Tenant selection rules themselves were changed more than once at the Boston Housing Authority, internally and by Housing and Urban Development (HUD) mandate, causing a lack of continuity and clarity. In this setting, conflicting desires by staff made compliance with existing rules less likely. Some of these contradictory objectives included integration, stability, housing those most in need, and maintaining the presence of higher-rent-paying families. In addition, area politicians as well as welfare staff exerted pressure on staff to house certain families immediately.

Housing authority staff were divided into several groups, chiefly reformers who favored integration and traditionalists whose vision of public housing remained caught in the original aims of the 1930s. Given these divergent directions, changing rules were often altered to suit certain preferences. For example, a staff member might immediately assign a family to a vacant unit after a call from an elected official. Or a manager might forget to offer a unit to a family he felt would cause problems. For projects in white areas of the city, a manager might be reluctant to admit too many black families, both for safety and to prevent resegregation. Yet the policy of the authority and of the HUD was to integrate without delay. Interestingly, the authority's top brass paid little attention to tenant selection. Change accompanied periodic HUD audits, but old patterns of selection soon resumed. It is only when the Boston Housing Authority is placed in receivership that tenant selection practices become more uniform.

As a contribution to the documentation of the use of discretion by street-level bureaucrats, *Breaking the Rules* serves a useful purpose. However, the book could have made a more comprehensive contribution to the social welfare policy literature as well. Although his examination was limited to tenant selection practices, Pynoos could have discussed the rationale for public housing and its shortcomings in Boston and elsewhere in greater depth. Pynoos occasionally does mention projects' faulty design, crime, vandalism, and other operational problems. A more complete treatment, however, would place rule breaking in Boston in a larger perspective and would provide more insight for those not already familiar with public housing. It would clarify the divergent concerns of central office staff and project managers regarding tenant selection.

Pynoos does not fully elaborate the policy area and he also does not provide a complete picture of the state of public housing in Boston. For example, the reasons for the Boston Housing Authority being placed in receivership—especially project and financial conditions—are not discussed in depth. If Pynoos had highlighted the external and internal environments of this public housing authority, it would have placed his findings concerning discretion in a larger context and made the book more useful to a larger readership.

Lack of clarity and varying motives and pressures can lead to a subversion of the rules in a bureaucracy with vague and contradictory goals. Pynoos makes this clear in his very detailed treatment of tenant selection practices in one authority. However, his work would have been strengthened by greater and more systematic attention to the policy and operating environments of the Boston bureaucrats he studied.

LANA STEIN

University of Missouri, St. Louis

Steering the Elephant: How Washington Works. Edited by Robert Rector and Michael Sanera (New York: Universe Books, 1987. xxi, 356p. $24.95, cloth; $12.50, paper).

Mostly written by conservative Reagan political appointees, this collection of essays tries to explain what the authors view as the failures of the Reagan administration to bring about radical policy change. Nine essays discuss urban enterprise zones, juvenile justice, education, and programs under the auspices of ACTION, the Legal Services Corporation, the Federal Trade Commission, the Small Business Administration, the Urban Mass Transportation Administration, and the National Oceanic and Atmospheric Administration. Eighteen other essays deal with the broader topics of political strategy, congressional and media relations, personnel staffing and administration, and coalition building.

Most of the essays are forward-looking; they seek to instruct future political conservatives in the next conservative presidential administration. How useful is the instruction? That depends on your view of U.S. democracy.

If you believe that those who elect a president are united in their commitment to an unambiguous policy course and that a president's ability to adopt change is not severely hampered by such things as statutory law, the principles of separation of powers and minority rights, and institutional checks and balances, then you will find this a revealing and infuriating indictment of the extent to which the Reagan revolution was betrayed. This resulted from Ronald Reagan's lack of awareness, courage, and skill, especially in screening the ideological correctness of the people he appointed; the White House staff's lack of political resolve in fighting for ideological principles; the politically liberal and self-serving civil service's ability to seduce presidential appointees and subvert presidential purposes; the willingness of Congress and the press to engage in character assassination; and from self-serving special interest groups' being artificially sustained by federal subsidies. Moreover, most of the contributors suggest that future conservative political appointees, forearmed by this knowledge of the conspiracy against conservative principles, should be more ideologically pure and less willing to compromise for the sake of getting things done. Instead, they should be secure in the knowledge that principled stands will redefine the political landscape and mobilize a conservative coalition strong enough to dominate it.

If, however, you believe that electoral mandates are ambiguous and that presidential powers are severely constrained, then you might find many of these essays to be either reassuring or frightening. They will be reassuring to those who profit from the status quo, since they should be more secure in their belief that ideologically conservative presidential appointees who follow the imperatives of these essays will remain blind to the need to become more politically astute and skilled as managers of policy change. They will be frightening to those who worry about what might be done by those who know they're right, who believe they are being frustrated by their enemies, who sense they are being betrayed by their friends, and who feel justified in circumventing both disorderly political practices and more orderly administrative and legal procedures.

PAUL A. DAWSON

Oberlin College

The Supreme Court and the American Family. By Eva R. Rubin (New York: Greenwood, 1986. 251p. $35.00).

In this volume summarizing the highlights of the decisions of the U.S. Supreme Court affecting the family in U.S. society, Rubin assesses both continuity and change in the jurisprudence of the high Court. What her work demonstrates is that the Court's role in family law has been much like the Court's role in social change in other respects; it generally lags behind societal changes but simultaneously allows such change to continue. Such a role is inherent in the nature of the function and power of the U.S. Supreme Court, indeed of the federal courts as a system. Policy issues most associated with social change are predominantly the domain of the state and local governments. Only when federal regulatory policies such as AFDC or Title 9 are implicated or one argues that local policies violate a *federal* constitutional right, do the federal courts have jurisdiction over these matters. And their function is, with very rare exception, only negative. A federal judge or the U.S. Supreme Court generally serves no greater role in the service of social change than to proclaim that legislative or administrative judgments are constitutionally flawed because they violate individual (or in the case of Rubin's work, familial) rights. But the courts have not and probably never will serve a substantially positive or proactive role in social change, particularly as it affects the family in the United States. For example, federal courts are unlikely to require that government at any level fund or encourage the building of child care centers, the critical family issue in the contemporary United States. In sum, the function of federal courts in support of family life in our system is fundamentally to set limits on legislative and administrative authority over the *privacy* of the family, so that it may function with relative autonomy. While this may, as an empirical matter, be the probable extent of the role of the federal judiciary, questions about how it exercises this authority must be asked.

Perhaps the most critical point in Rubin's book is her analysis of the Supreme Court's very narrow, very prejudicial, and very outdated conception of the U.S. family. Unmarried people living together, with or without offspring, do not qualify; students living together do not qualify; gay and lesbian couples do not qualify; old or impoverished people living together to survive economically, emotionally, or otherwise also do not qualify for the protections of familial relations. While persons living in such arrangements do not necessarily lose their civil liberties under the Constitution and while, as Rubin points out, they often win their battles for food stamps and other governmental services on *other* grounds, their right to function under the protections of the status of family in the eyes of the government is by and large not recognized by the U.S. Supreme Court. The Court's avowed preference in its decisions and especially in its dicta, is for the family of our folklore, a ceremonially married man and woman with only their own shared biological offspring, in which the male is the sole breadwinner and the female runs the home. As recently as 1961 in *Hoyt v. Florida*, the high Court upheld the exemption of women from jury service on the basis of their responsibility for maintaining the household, a responsibility that the Court found arguably to be incompatible with jury service.

When analyzing familial rights, one necessarily analyzes issues of sexual equality as well. While *Hoyt* was paradigmatic of its time in the circumscribed role perceived for women as civic actors in society, more important to Rubin's work is the effect of Supreme Court decisions on the relations *between* the sexes in family life. She quite correctly identifies the protection of the right to reproductive choice as supported by the Court in *Roe v. Wade* (1973) and its progeny as the critical women's rights issue in the relationship of women to the government. Implicit, however, in her assessment of the effect of the abortion decisions on family is that *Planned Parenthood v. Danforth* (1976) was far more important. In that decision the Court prohibited the states from requiring the consent of a woman's husband before she could obtain an abortion and it was also the first in a series of decisions that have substantially narrowed the role of parents in the reproductive choices of their minor daughters. In sum, while the Court's decisions determining the limits of the authority of the government over familial matters are important, its decisions that broaden *individual* rights against the government may ultimately have far more impact on the relations among family members and may slowly redefine those power relations. Thus, those who want the traditional family hierarchy preserved—and, therefore, want government limited in its authority over the family—must, paradoxically, support greater governmental authority over various areas of *individuals'* lives, particularly adolescent children. Among the relevant areas cited by Rubin are adolescent sexuality (including access to contraception and abortion rights), school discipline (including restrictions on the Fourth Amendment right of students to privacy in their lockers, purses, and so on), the return of prayer in the schools, and the censorship of reading matter deemed to be offensive to fundamental values.

Rubin has provided a very wide ranging study of the ideology and impact of Supreme Court decisions that have implications for family life and especially for traditional familial patterns of patriarchy. It would serve especially well as an introduction to the subject for someone who wants to see all of the disparate pieces drawn together.

GAYLE BINION

University of California, Santa Barbara

Why Presidents Succeed: A Political Psychology of Leadership. By Dean Keith Simonton (New Haven: Yale University Press, 1987. xi, 292p. $22.50).

The author of this book is a psychologist who wishes to develop a "political psychology of the presidency" consisting of analysis of the personal attributes that explain effective leadership, the situational constraints on leader potential, and the attributions that followers and observers make in judging the performance of leaders. He applies personal, developmental, and social psychology to political phenomena to develop a transhistorical quantitative framework of prediction in which to assess the relative strength of individual and situational factors in the determination of successful leadership. There are four tests of presidential success: winning elections, popularity in the polls, administrative performance, and evaluation of historical greatness. Empirical research on each of these themes is analyzed.

The analysis of who wins presidential elections and why is a straightforward summary of political science research. The factors that explain victory are personal attributes, campaign rhetoric and strategy, and the match of these things to the "political zeitgeist" (not the most precise phrase). Along the way we are given some trivia about height and birth order as they contribute to success. It is concluded that situational factors, especially war and peace and the state of the economy, override personal characteristics most of the time in explaining victory and defeat, but suggested that personal characteristics are congruent with the historical situation. Thus the power-maximizing leader is better suited to crisis than is the leader with affiliation needs.

There is a useful analysis of the research literature about the decline of presidential popularity with the conclusion that international crises do rally support, that prolonged wars are damaging, and that inflation—which affects all voters—is more damaging than unemployment, thus putting Democrats at a disadvantage. Publics sometimes punish presidents for the sins of their predecessors and hold presidents responsible for events they cannot control. Situation overrides personality, although some presidents may act to get themselves in trouble.

Effective administrative performance is a function of the match of historical situation to presidential characteristics. For example, power-maximizing presidents—who are first or only children and whose views match those of a majority of voters—will lead effectively and be prone to international expansion in a period requiring strong government. Again, we see congruence. The key to success is to be the right person at the right time.

The author loses perspective in the chapter on presidential greatness. He distills several polls of historians to achieve a consensus ranking on the great presidents, and then analyzes those presidents to develop five predictors of greatness, which he would apply to future presidents: record as a war hero before becoming president, years in office, engagement of the country in a war, the absence of scandal, and assassination. This is a transhistorical analysis that deliberately ignores unique factors in performance or attribution. The difficulty with the predictive equation is that the five predictors do not tell us why Lincoln, FDR, and others are at the top. Unique achievements account for the ranking and the shared characteristics mean little by themselves. The author's transhistorical method fails here.

The final chapter is an attempt at a theory of political leadership in which leader characteristics, situational variables, and follower attributions are joined. The analysis is more of a summary of findings than a theory; as the author admits, it is scaffolding for a future theory. There are interesting insights about the degree to which contemporary and historical attribution override actual personal characteristics or performance as presidents become symbols of an age.

My first reading of this book was unfavorable. I reacted negatively to the silliness of the inquiry into birth order, height, and so on, and to the crudity of some of the transhistorical measures. A second reading persuaded me that this is a serious effort to integrate the individual, the situation, and historical action. Presidential scholars will find that most of the conclusions match their intuitions. But idiographic analysis, the author's term for most presidency research, would perhaps not have developed such data and propositions within a theoretical framework.

Much of the presidency is suitable for nomothetic study (the author's term), inasmuch as quantification of regularities is feasible. This includes voting, public attitudes, and legislative support data, those aspects of the presidency in which large numbers and aggregate measures are relevant. However, decision making and administrative behavior are less susceptible to such analysis for at least two reasons: the evidence is not easily quantifiable, and the primary data are gestalts, whole stories, that are distorted if disaggregated. Nomothetic and idiographic strategies of research are complementary, the former providing structures of the political dynamics within which decision making takes place and the latter filling the black box of policy-making within the nomothetic framework. Thus, case studies of economic policy-making explain best when set within a framework of economic policy and popular politics provided by quantitative analysis.

There is a tension today within political science between the positivistic, behavioral approach to knowledge and the phenomenological approach. Study of the presidency suggests how these two approaches might be brought together constructively.

ERWIN C. HARGROVE

Vanderbilt University

Breaking the Impasse: Consensual Approaches to Resolving Public Disputes. By Lawrence Susskind and Jeffrey Cruikshank (New York: Basic Books, 1987. xi, 288p. $19.95).

The Politics of Environmental Mediation. By Douglas J. Amy (New York: Columbia University Press, 1987. x, 255p. $30.00).

Professional mediation emerged as a highly touted alternative to adversarial politics in the early 1970s. Proponents suggested that innumerable conflicts—from landlord-tenant disputes to environmentalist-developer confrontations—could be resolved through refereed discussions. Interest groups could get beyond antagonism, stereotyping, and misunderstanding; identify their similarities and compromise their differences; and settle their disputes without the costs or coercion of government arbitration.

Lawrence Susskind and Jeffrey Cruikshank's *Breaking the Impasse* is a sophisticated book advocating professional mediation. It tells of the promise and practice of negotiating consenses. Douglas J. Amy's *The Politics of Environmental Mediation* takes the standpoint of scholarly skepticism. It dissects the problems and prejudices in mediating environmental conflicts. The explicit debate here is over the benefit-cost ratio of professional mediation. That is the strength and weakness of both books.

Susskind and Cruikshank begin with the suggestion that U.S. politics is at an impasse. For every interest, there tends to be an equal and opposite interest, which often produces political gridlock. Judges, elected representatives, and public agencies are notoriously bad at settling these confrontations. If they are willing to render decisions, politicians' solutions rarely satisfy the disputants, usually prolong the battle and its costs, and almost always erode public confidence in government.

The authors' alternative is "negotiated approaches to consensus building." These negotiated approaches do not absolutely require professional mediators, but Susskind and Cruikshank, connected with the MIT-Harvard Public Disputes Program, clearly believe that the presence of a neutral expert is essential in most cases. He or she can help opposing interest groups meet informally and transform their antagonism into "integrative bargaining." That involves changing a zero-sum conflict into an "all-gain," or "win-win," solution.

For example, citizens' groups, developers, and local government officials might identify their underlying common interest in a community's quality of life and prosperity, trade off zoning variances for community amenities, and arrive at a "responsible" development agreement that is then ratified by the appropriate government agency. Susskind and Cruikshank provide numerous examples of *apparent* public impasses where voluntary mediation generated creative consensus and all-round contentment.

Most of their book is a how-to primer intended for professional mediators and potential parties to mediation. When is a mediation process likely to be successful? What is the role of the neutral expert? How can a powerless citizens' group use mediation to overcome an imbalance in resources, technical expertise, and political clout? What do political officials have to gain from informal dispute resolution? Why do businesses have a long-term interest in settling through the consensus process? How can pitfalls be avoided and creative cooperation conceived? People considering a negotiated settlement over litigation, for example, should read this book.

But they should also read Douglas J. Amy's *The Politics of Environmental Mediation* before making a decision. Amy skillfully presents the arguments for informal mediation and then shows that the practice of negotiated settlement seldomly delivers on its promise. On the one hand, conventional adversarial politics is not as forsaken as professional mediators imply. It is often the most advantageous arena for environmental groups. On the other hand, mediation as a *neutral* mechanism for conflict resolution is a myth. Amy shows in detail how powerful interests use mediation to force and cajole their weaker opposition into "sweetheart deals." He, too, draws on a wealth of recent examples.

Environmental groups are usually the weaker parties in negotiated settlements. Their power deficit means that some of them, especially the least compromising, are ignored in mediations. Those invited to discussions have minimal resources for hiring professional negotiators, bringing in technical experts, or gaining access to proprietary information so important in environmental disputes. These groups are also vulnerable to pressures to settle for far less than what might be considered just because their powerful business opponents can blackmail them with threats of prolonged litigation or perhaps powerful lobbying campaigns. Amy states, "It is clear that unequal power between participants in environmental mediation can undermine the extent to which this process is representative, fair, and voluntary."

Can professional mediators right the imbalance as Susskind and Cruikshank suggest? Amy is doubtful. Professional mediators have a stake in convincing disputing groups that everyone can gain by joint problem solving and splitting residual differences. In practice, this means that mediators pressure environmental groups to be "reasonable"—for example, to agree to accept a "controlled" public health risk in return for limited safeguards subsidized by business. Such a trade-off is questionable; environmental groups must cede a basic principle while business spends a few dollars extra. And as long as mediation proceeds on a case-by-case basis, citizens' groups have to negotiate each conflict anew, business faces no systematic restraints, and professional mediators are guaranteed a steady stream of clients for their services.

Amy argues that mediation is a structure of "political control" used by industry to manage the opposition. It can usually be seen as "an effort to disempower environmentalists" by distracting them from more effective political strategies like litigation. From an environmental standpoint, mediation is promising only when there is a clear balance of group power that fosters stalemate. But given the imbalance of power, recently skewed further by the Reagan administration, negotiated settlements are increasingly a form of "second-class justice."

Susskind and Cruikshank do not necessarily disagree with this assessment. They, too, recognize that power imbalances prejudice negotiated settlements, and they note that mediation is not appropriate when moral principles rather than resource distributions are at stake. But their optimism that voluntary agreements generally "offer the wisest, fairest, most efficient, and most stable outcomes possible" involves overlooking the systematic inequalities in the pluralist marketplace and the moral stakes inherent in distribution disputes. In my view, Amy is the better political analyst.

To summarize, Susskind and Cruikshank treat informal, face-to-face mediation as a form of participatory democracy that can supplement and humanize U.S. pluralism and adversarial politics, while Amy sees mediation as a form of pseudoparticipation that reproduces and legitimates pluralist inequalities that are better addressed in courts and legislatures. Neither book, however, shows much sensitivity to democratic political struggle and protest by citizen groups as perhaps a more important determinant of outcomes.

Frances Fox Piven and Richard A. Cloward have pointed to the significance of mass disruption in forcing the United States' powerful interests to grant concessions. My own studies of progressive politics suggest that failure to sustain citizen pressure and protest virtually guarantees a loss of concessions. Whether the concessions are negotiated through professional mediation or conventional political institutions may be a secondary strategic concern. Both *Breaking the Impasse* and *The Politics of Environmental Mediation* provide excellent analyses of the benefits and costs of mediation. But both are so concerned with "pragmatic" resolutions of conflicts that they have ignored the citizen disorders that make democratic politics "work."

MARK E. KANN

University of Southern California

Why Americans Don't Vote: Turnout Decline in the United States 1960–1984. By Ruy A. Teixeira (Westport, CT: Greenwood, 1987. viii, 149p. $32.95).

This excellent little book adds a great deal to our understanding of the perplexing problem of declining turnout in U.S. presidential elections. Ruy Teixeira, who was a graduate student in sociology at the University of Wisconsin when he wrote the book, brings the perspective of a sociologist to the study of nonvoting. He pays careful attention to demographic factors (such as age, race, and region) and includes new hypotheses that might account for the "disconnectedness" of people from the political system. In particular, he raises the possibility that the marked decline in "rootedness" in U.S. society (as measured by the decline in people married and living with their spouses and the increase in residential mobility) could bring about less political involvement.

Yet Teixeira is also well aware of the political science findings regarding turnout and includes three important "sociopolitical" variables in his study: partisanship (strength of party identification), efficacy, and campaign newspaper reading. The latter variable has not been included in many turnout studies and is particularly interesting.

Teixeira skillfully and effectively employs probit analysis as his major methodological device. The data source is the Michigan National Election Studies. Although all tables in the book present findings in probit format, even readers unfamiliar with probit analysis will find the book understandable; mathematical coefficients are given clear verbal interpretations throughout.

Beyond his skillful use of probit analysis, Teixeira makes two major methodological contributions to the study of the decline in turnout between 1960 and 1980. First, he builds into his model a method of accounting for the rise in educational levels in the population—a rise that should produce higher, not lower, turnout. Second, he examines the possibility that not all variables were acting in the same way during this entire 20-year period. In other words, he controls for something he calls "periodization" by examining the decade of the 1960s and the decade of the 1970s separately. Both of these methodological insights produce findings that help us come closer to solving the turnout puzzle.

In all, Teixeira's model explains 88% of the 1960–80 decline in turnout. This is an impressive result. The political variables explain more than the sociological variables, by a ratio of about 5 to 3. Yet, without the sociological variables, clearly, much less of the decline in voting turnout would have been explained. To say anything more about the findings in this short review would not do justice to the complete picture that Teixeira's analysis offers. The book should be read to fully appreciate that many different variables were important in explaining the decline in turnout and to see the different effects of the variables in each of the two decades.

Teixeira adds a chapter on the 1984 election as a kind of postscript at the end of the book. In 1984 there was a slight increase in turnout. This increase would be expected, given the change in distribution of some of the key variables, particularly the marked increase in political efficacy. Applying his model, Teixeira finds that turnout should have gone up even further than it did. Apparently new, unexplained, factors are having a more decided effect on turnout in the 1980s. And so there is need for still more research into this intriguing problem.

My only major criticism of the book is that Teixeira's historical and political interpretations often show a lack of thorough understanding of the events and problems of the time and a failure to comprehend the nuances of political phenomena fully. For example, the treatment of political parties and campaigns is very rudimentary. However, readers can supply their own interpretations, since the variables are laid out so clearly.

In all, this book makes an important contribution to our understanding of turnout. It examines an unusually comprehensive set of variables and handles the data skillfully and with insight. It should have a wide audience.

DAVID E. REPASS

University of Connecticut

The Constitution under Pressure: A Time for Change. By Marcia Lynn Whicker, Ruth Ann Strickland, and Raymond A. Moore (New York: Praeger, 1987. vii, 223p. $38.95, cloth; $14.95, paper).

"Never," the authors write, "has the time been more propitious to examine the basic fundamental structures of U.S. government." To this end their book evaluates the Constitution's capabilities viewed from the demands placed upon it by contemporary circumstance, with special attention given to its role in shaping government and representational issues. The study is aptly historical, beginning with an examination of British misrule in the colonies, the failings of the Articles of Confederation, and a recitation of the conflicts played out in the Constitutional Convention. In an important chapter the authors then survey the political ideas upon which the constitutional enterprise was erected, including the principles of individualism, private property, contracts and law, freedom and equality, and democracy, followed by a statement of liberal constitutional principles. A rather brief section relates capitalist thought to these constitutional principles. This historical background is followed by chapters on federalism, the courts, and the amendment process, all of which are arranged to emphasize historical sequences. The final three chapters are more analytical and focus on the major evaluative effort of the work. These deal substantively with issues of representation (chap. 6), presidential versus parliamentary government (chap. 7), and policy specialization (chap. 8).

For a multiauthored work, the book reads well. It is nicely organized as a teaching text. For example, all chapters have tables that summarize the text in useful fashion. Overall, the chapters are constructed to familiarize the new reader with the major issues to which each chapter is devoted. Descriptive historical material is supplemented with interpretive material drawn from the constitutional or institutional literature. The interpretation of the amendments (chap. 5) in the context of the social transformations that either gave rise to them or have come to influence their application is especially good. All chapters end with a reference section.

The last three chapters are more explicitly analytical, and it is also here that one feels the book loses some of its thread. Chapter 6 is devoted to a discussion of representation and begins in the manner of the other chapters, surveying various issues and criticisms of representation (for example, the philosophical base, constitutional limitations on participation, direct election of senators, the electoral college, and so on). After a brief discussion of parties it moves with stylistic abruptness to a rather detailed discussion of voter apathy and a conceptual scheme for linking public opinion, legislative issues, and representation. The style of analysis here is quite out of character with what precedes it and not well integrated with what follows. Chapter 7 is written in the style of the first five chapters, and weighs presidential-style government against the presumed virtues of parliamentarianism; chapter 8, in the style of contemporary policy literature, offers five criteria for a good legislature and then evaluates Congress according to these criteria.

The book ends without a concluding chapter that leaves one with the sense of its having run down without fulfilling the promise of its theme. The integration provided in the early chapters by the historical experience of the Constitution does not seem to be matched by the choices made toward the end of the book as to what should be emphasized and for what purposes.

A more serious difficulty is the overall relationship of the book to its stated intents, signaled by the urgencies of its title. It never finally comes to grips with the task of examining the Constitution in light of the massive social changes of the past two hundred years. The predominant emphasis is on "institutional" analysis, while the burden of the argument would appear to lie more in the direction of relating the constitutional structure to the transformations of the political economy, both domestic and international. The closest approach to this task comes, as indicated, in the chapter on the amendments in which they are seen as critically related to the demands placed on the governmental structure by a changing social fabric. By contrast, the second chapter, entitled "Liberalism and Capitalism," contains only five pages on capitalism and two paragraphs on contemporary capital. It is fair to say that the subject is not really discussed. One wonders what "pressures" upon the Constitution are referred to in the title. A significant amount of current U.S. politics and policy literature would emphasize the political economy, especially the role played by increasingly large and concentrated wealth structures. More recent literature would emphasize the equally powerful impacts on U.S. political and social life of a world economy into which the U.S. is fully insinuated but the dynamics of which it can no longer wholly control. Yet other literature would emphasize the emergence on the world scene in the past decades of transformative institutions such as the transnational corporation, a new manifestation of capitalism that has an impact on all governments, democratic and nondemocratic alike.

None of these themes, however, are taken up in this book, and while it is not totally fair to hold against a book what it does not do, one is led to in this case by the professed intentions of this volume. Viewed outside this frame and within the more limited boundaries of a text devoted to a recitation of constitutional issues in government, it remains a useful and welcome book. Viewed from the perspective of its ambitious intentions, it is too limited a treatment.

DEANE NEUBAUER

University of Hawaii

COMPARATIVE POLITICS

Tanzania: Crisis and Struggle for Survival. Edited by Jannik Böesen et al. (Uppsala, Sweden: Scandinavian Institute of African Studies, 1986. 325p. $26.50).

Once hailed as an examplary case of how to pursue development because of its emphasis on equity within the context of an Africa-inspired strategy *(ujamaa),* Tanzania often suffers rebuke today. In little more than a decade the pendulum has swung from one extreme to the other. Because of its political stability, relative openness, and innovative development policies, Tanzania received more scholarly attention than most other African countries in the 1960s and early 1970s. In recent years, however, its position in the comparative politics literature has suffered a serious decline. Although it seems that permits to carry out research have been easier to obtain for expatriate scholars in the past five years, other factors, notably disillusion with the *ujamaa* experience and logistical problems, have limited the production of academic books on the country. This trend has been particularly apparent in North America.

It is in that perspective that this book deserves attention by scholars interested in comparative development experiences. It is the first solid scholarly assessment for many years of Tanzanian policy. The authors are all social scientists from the four Nordic countries—Denmark, Finland, Norway, and Sweden—some of Tanzania's strongest supporters in the international community. Their account, however, is no apology for the way the Tanzanian government has pursued its development strategy in the past 20 years. Examining one sector after the other, the volume conveys a penetrating critique of much of what was being done in the name of *ujamaa* in past years.

Each chapter tends to follow the same format: a presentation of how the crisis manifested itself, how it came about, its roots, and the response attempted by the Tanzanian authorities. Though the authors do not follow an identical conceptual framework, they tend to agree that the real root of the present crisis lies in the failure of the government's agricultural policy. Here the book tends to follow the argument of other critics of agricultural policies in Africa, notably Robert Bates and Michael Lofchie, but goes into greater detail in its analysis of various aspects of both the agricultural and the industrial sectors. Knud Erik Svendsen—once the economic advisor to President Nyerere—provides the macroeconomic analysis that helps place the various case studies in a broader perspective.

The book has two principal shortcomings. While it does provide valuable insights into the outcomes of specific policies, it fails to analyze the political structures in which policy-making and implementation take place. The authors are aware of this gap in the analysis but they provide no clue as to why they fail to provide a discussion of the Tanzanian state and its decision-making structures. As such, the book lacks the elements that a political scientist would particularly look for in explaining policy.

Another shortcoming is the limited attention the book pays to the social sector. Much of Tanzania's performance record to date is associated with creative and innovative policies in the health and educational sectors. Few countries have made as much progress in adult literacy as Tanzania has in the past two decades. How has the crisis affected these achievements? How much of the social sector is holding up in a situation of budgetary retrenchment? What alternative institutional arrangements, if any, are being considered? The picture of Tanzania in crisis remains incomplete without further discussion of these questions than the book provides.

It must also be noted here that although the authors have made an extra effort to get their research published without delay, the book is already overtaken by one important event: the final agreement between the International Monetary Fund and the Government of Tanzania to restructure the country's economy. This accord has changed the basis parameters of policy beyond what the authors of this volume are considering.

In spite of these limitations the book is the most thorough and up to date overview of Tanzanian development policy currently available. It lends itself very well to use in development policy seminars. Finally, a compliment to the sponsoring institution of this project, the Scandinavian Institute of African Studies. After a lull in its seminar and publications effort, it has more recently returned to the academic publishing scene with several valuable monographs and issue-oriented volumes, for example, on African refugees. The book about the crisis in Tanzania is another confirmation that the research it sponsors is worth taking seriously.

GÖRAN HYDEN

University of Florida

Muslim Hausa Women in Nigeria: Tradition and Change. By Barbara J. Callaway (Syracuse, New York: Syracuse University Press, 1987. xx, 242p. $29.95).

This book is a detailed study of the Hausa Muslim women in Kano, northern Nigeria; Kano is the largest state in the Nigerian Federation. Muslims represent the largest minority (38%) in Nigeria, and 10 out of 19 states have Muslim majorities (p. 103). The study asks whether education and political activism would affect the culturally assigned roles of women. It concludes in the negative; the overwhelming power of religious beliefs circumscribe the potential for change.

The strength of this study lies in the rich data, drawn from field work and extensive survey research, collected between 1981–83. Fascinating insights are offered on women's views of education, polygamy, marriage, divorce, and religion. The contrasts between male and female views on a specific subject and the two sexes' perceptions of each other are interesting. The Hausa society is disheartening to any feminist and humanist; this is a tightly secluded and segregated society with high infant and female mortality rates. Most girls are married by age 12 and life expectancy for women is only 36 years as compared to 45 for men (pp. xvi, 36).

Many of Callaway's findings are intriguing. For instance, the two sexes often use different terminologies to describe a single woman who is not a member of the family. While the women's reference clearly means nonmarried, the men use a term which translates into prostitute, implying sexual and economic independence (pp. 42–43). After a detailed discussion of the role of reformist male leaders, especially Mallam Aminu Kano and Isa Wali, who advocated Koranic principles of justice and equality for both women and men, the author states that reform in the status of women is more likely to succeed if urged by men (p. 129). This is a common view, and it stems from the frustration borne by reformists who wish to bring about change in abusive cultural practices. A similar opinion was expressed to this writer by a Sudanese female physician who had actively campaigned to stop female circumcision in her country to no avail.

Certain themes in this study can hold cross-national relevance in other Islamic societies, such as the strong belief in destiny, closeness of women to each other, old age as a variable establishing a female's legitimate authority and relative independence, and deep distrust of Western ethics.

Despite these possible similarities one must be cautious not to generalize the Hausa case to other Islamic societies; regrettably, Callaway attempts to do just this in the last chapter by presenting Islam as the central theme. Some of these generalizations are sweeping and not supported by available evidence (pp. 191, 202, 205–7, 209). Hausa demonstrates an extremely closed society; it is a classic case of women in purdah where deviations are hard to come by. Not even the Islamic Republican regime in Iran has been able to exert such a tight control on its female population. The State of Kena appears highly reactionary and static. While the basic precepts are Islamic, the unbending severity and rigid adherence to implementation is unusual. Perhaps what makes the Hausa Muslims so strict is their forced coexistence in the Nigerian Federation where there are large numbers of non-Muslims. This self-preserving tendency might have strengthened their continual adherence to the most conservative Islamic schools of thought. Also, changes involving women are very new in comparison to many other Islamic countries. Universal primary education was only introduced in

1976 (p. xv), women voted for the first time in 1979 (p. 101), and there was no family planning in the area until 1983 (p. 28). To generalize from Hausa's Islam to other societies is to miss the complex economic, sociopolitical, and nativist elements that shape a national milieu.

The book states in error that polygamy has been prohibited in Jordan, Syria, Morocco, Iran, and Pakistan (p. 188). These countries have devised bureaucratic means to discourage and restrict the practice of polygamy, not to outlaw it. Also, it was Iraq, not Iran, that passed a personal status law restricting polygamy in 1959; Iran followed suit in 1967 and later in 1975. Since the inception of the Islamic Republic, the government has officially encouraged polygamous marriages providing incentives and labeling it as an act of heroism and devotion to Islam.

Despite these shortcomings, Callaway's work is impressive. This book makes a very important contribution to our knowledge of women in a closed Islamic society and makes a statement that *change* is a concept more liberally used in theory than applied in practice.

ELIZ SANASARIAN

University of Southern California

Ideological Conflicts in Modern China: Democracy and Authoritarianism. By Wen-shun Chi (New Brunswick, NJ: Transaction Books, 1986. xiii, 372p. $39.95).

China's Establishment Intellectuals. Edited by Carol Lee Hamrin and Timothy Cheek (Armonk, NY: M. E. Sharpe, 1986. xix, 266p. $35.00, cloth; $14.95, paper).

These two books examine the relationship of Chinese intellectuals to authority. The late Wen-shun Chi resurrects the variety of ideological viewpoints eclipsed by communism and compares what he sees as a consensus informed by a synthesis of traditional Chinese ethics with Western liberalism against Mao's political thought. Carol Lee Hamrin and Timothy Cheek and others analyze the roles and careers of intellectuals who found a place on the Communist rostrum only to fall victim to some ideological dispute.

Kang Youwei and Liang Qichao, with whom Chi begins his discussion, both belong to the first generation of Chinese intellectuals exposed to Western political ideas. He goes on to discuss the careers and ideas of Sun Yatsen, Hu Shi, Zhang Junmai (Carsun Chang), Zhang Dongsun, Liang Shuming, and Chen Duxiu, as well as Mao. Chi sees the essence of early twentieth-century Chinese political thought as a search for democracy. This is not entirely convincing, for his sample includes both unabashed liberals such as the student of Thomas Dewey, Hu Shi, and the conservative Confucian reformer Liang Shuming, not to mention the Confucian-inspired visionary Kang Youwei, or the radical Chen Duxiu. As a symptomatic Chinese scholar, Chi implicitly assumes that liberalism is compatible with—if not actually inspired by—the Chinese ethical tradition reaching back to Mencius. Not surprisingly he reviles Mao's effort to uproot that tradition.

Chi is most clear-headed in his analysis of the failure of these intellectuals to secure a liberal outcome. They were still animated by a desire to be "tutor to the king" as a method of bringing about good government and were themselves indifferent politicians. Mao, on the other hand, was a man of action. While the Communists organized and strived to realize their ideals, "the reformers of the scholar group just talked and waited for competent leaders to invite them to advise and adopt their programs" (p. 292).

Chi deplores the tragic consequences of the revolution, and intends to redress the historical balance by exposing Western readers to modern Chinese political thinkers who were not "revolutionary ideologues." His book, in addition to reacquainting us with some neglected Chinese thinkers, helps correct a perspective that sees the past century of Chinese history in exclusively revolutionary terms. Indeed, some of the thinkers discussed may yet find a new audience in the current era of reform.

The eclectic selection of representative intellectuals in the Hamrin and Cheek volume ranges from apparatchiks like Peng Zhen to the writer Bai Hua. Somewhere in between sit the historian Wu Han, the editor and essayist Deng Tuo, the Marxist philosopher Yang Xianzhen, the economist Sun Yefang, and the readers and editors of *China Youth.*

The introduction by Cheek and Hamrin provides a sensitive analysis of the role of senior intellectuals in the Chinese Communist regime and puts the selected characters in context. One may dispute calling the lifelong party cadre Peng Zhen an "intellectual" but cannot dispute his importance, especially in a volume treating his close intellectual and political associates, Wu Han, Deng Tuo, and, to a lesser extent, Yang Xianzhen. Pittman Potter's chapter treats Peng's legal-political ideas in a way that clearly sums up his steadfast Leninism. Carol Lee Hamrin's portrayal of Yang Xianzhen highlights an orthodox Marxist philosopher whose academic views were enmeshed in personal and national political rivalries out of all proportion to their substantive merit. He is a victim of a system where ideological and personal authority became synonymous. Following his rehabilitation in 1979, Yang turns into a curious anachronism, a rehabilitated victim who persists in wielding the stick of Marxist orthodoxy against his erstwhile persecutors now turned reformers.

Deng Tuo, the founding editor of the *People's Daily*, emerges from Timothy Cheek's sympathetic portrait as a "Mozartian commissar" who combined strict orthodoxy with sensitivity and erudition. Deng Tuo, a Marxist reincarnation of the upright Confucian scholar, reinterprets Mencius's "kingly way" as "the honest ideological workstyle of a mass line based on practical reality," while the "tyrannical way" was "the blustering ideological workstyle of willful acts based on subjective and arbitrary decisions." The latter "workstyle" drove Deng to his death.

Barry Naughton's Sun Yefang comes closer to the Western ideal of the principled scholar's allegiance to the truth. He consistently defended the objectivity of the economic discipline against the arbitrary voluntarism of Mao. Secure in his Marxist convictions, Sun saw no contradiction between his principles and loyalty to the party.

Tom Fisher emphasizes Wu Han's attachment to the concept of *ging guan* or "upright official." Yet Fisher dismisses the Maoist accusation that Wu wrote *Hai Rui Dismissed from Office* as an Aesopian protest against the sacking of Peng Dehuai. Fisher shows that Wu's interest in the Ming official predates the Lushan Plenum, and notes the strict adherence to historical fact in the play. The attack on Wu was "a clear case of the 'manufacture of deviance,'" (p. 175) and Fisher concludes Wu was in no way a dissenter.

Richard Kraus depicts Bai Hua as a "cultural gadfly" who never loses sight of his political patrons. Kraus applies the model of the poet-official Qu Yuan to contrast the Western liberal intellectual and the Chinese scholar. Bai Hua seeks to maximize the power of artists to influence state policy rather than apply the liberal prohibition against state interference in the arts.

David Ownby's essay on youth provides the epitaph for the Confucian pattern of scholarly political engagement in both these volumes. Communist intellectuals, like their Confucian forebears, saw a moral obsession to find self-fulfillment in service to the state. In post-Mao discussions on the meaning of life in *China Youth*, Ownby finds the generation who grew up entirely under Communist rule and experienced the Cultural Revolution cut off and alienated from this tradition. This generation seeks fulfillment in the private sphere, completely detached from state concerns.

Ultimately the outcome Wen-shun Chi desired may be better served by this ethic. The urge to serve the state rendered efforts at moral suasion over Mao (or Chiang Kai-shek) futile. By rejecting the role of adjunct to the state the new generation may be better able to turn the state toward their own purposes, an ethic much closer to liberalism than Confucian morality. *China's Establishment Intellectuals* thus answers the question of the failure of liberalism much better than did Chi, who posed it. Nevertheless, both books enrich our understanding of the relationship of intellectuals to authority in modern China.

JEREMY T. PALTIEL

University of Alberta

Professions and the State: The Mexican Case. By Peter S. Cleaves (Tucson: University of Arizona Press, 1987. xv, 147p. $19.95).

The selection of Carlos Salinas de Gortari to be Mexico's next president underscores a significant trend in that nation's politics: the saliency of "modern" professional training. Mexico's political elite traditionally added extensive experience in party and elective positions to its law education at a Mexican university. Salinas earned a masters and doctorate in economics at Harvard University; his professional expertise, coupled with relatively little political experience, promoted his meteoric rise. Although other authors have already documented the turn from law toward economics, Peter Cleaves provides new insights into this and other tendencies concerning professions and the state.

The interesting core of Cleaves's argument is that relations between professions and the state in Mexico, as well as key issues confronting those professions today, cannot be understood according to the standard liberal model of the politics of professions. That model posits professions with special characteristics (skills, prestige, norms, monopolies on knowledge, gatekeeping functions, cohesiveness and so forth) that give them considerable power, including influence over the state; private practice and direct professional-client relationships are common. In Mexico, however, professions typically lack those special characteristics. For example, employers are the gatekeepers determining who among the professionally uncontrolled supply of university graduates will find desired employment, and government is the dominating employer. Overall, Mexico's professions lack the prestige and organizational power to control their own destinies, not to speak of influencing state policy. It is the state that powerfully shapes the professions.

This is stimulating stuff. It runs counter to important thought about development and therefore to policies of organizations trying to shape professional development in the third world. Cleaves himself, who spent 10 years working for the Ford Foundation in Latin America, tentatively takes a "pro-profession" (p. ix) view and uses terms such as professional "preconsciousness," but he does not necessarily endorse the liberal model (or any clear alternative).

The approach is original; so is much of the information on professions per se, whereas information on the political and university systems is sketched in. Other works on Latin American politics touch on the professions, but Cleaves makes the study of professions central. He identifies several useful analytical dimensions. A good example is his national-international cleavage. Mexico's professions differ among and within themselves according to where their members were trained, whence they draw their inspirations and research agendas, and so forth. Foreign assistance has naturally promoted the internationalist side, with both positive and negative consequences for Mexico.

To explore such dimensions, Cleaves chooses five professions critically intertwined with the state: law, economics, agronomy, petroleum engineering, and medicine. Comparisons among the five at least partly offset the lack of cross-national comparative scope. That lack is understandable in terms of the lack of parallel work on other underdeveloped nations. Still, the books's findings would have taken on more meaning if they had been set alongside even nonsystematic comparisons to Latin American nations. What happens where the state has been less stable and civil society has been stronger? Similarly useful would have been a closer look at state-profession relations in European nations from which Latin America drew its model of professional training in public universities, nations that do not conform to the liberal model based on the U.S. and British cases (as Cleaves recognizes).

Indeed the big problem with this work is its limited scope. It is so limited as to appear more like a rich essay than a book from a major press. Cleaves manages to say quite a bit in 105 pages of text, based on 43 interviews. More interviews, information from other sources, data on professional association characteristics, and contextualizing and comparative information from other works on professions and on Mexican politics and higher education could all have helped substantiate, elaborate, and delineate the findings. They might even have helped suggest an alternative model of relations between professions and the state, beyond the critique of the liberal model.

Several other reservations might be raised. For example, based on Cleaves's own evidence I would see somewhat more strength in Mexico's professions than he does and thus less consistency with certain interpretations of Mexican authoritarianism. But such reservations are legitimately debatable in a good work and pale by comparison with the limitation concerning scope.

Perhaps thinking of the book's limited scope, Cleaves set a modest standard of success: to spark "additional interest in a fascinating subject" (p. x). That standard he met, and more. Cleaves brings fresh perspectives and information to the topic, and he delivers his material in a well-written and judicious manner. The book may disappoint for what it does not cover; it impresses for what it does cover.

DANIEL C. LEVY

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China's Continuous Revolution: The Post-Liberation Epoch, 1949–1981. By Lowell Dittmer (Berkeley and Los Angeles: University of California Press, 1987. xv, 320p. $35.00).

This is a broad-ranging and learned interpretation of the first 32 years of the People's Republic by one of the more reflective observers of the Chinese scene. A book that will be appreciated most fully by the seasoned China specialist, it contains Dittmer's detailed analyses of three crucial periods in the party's unusually long effort to "continue the revolution": 1955–60, when the party, having replaced old social structures with new socialist ones, flirted briefly with liberalization, finally lurching back toward repression and mass mobilization; 1964–67, in which elite polarization spawned a revival of Mao's charisma and a destructive and incoherent mass movement; and 1968–76, in which radical triumph gave way to elite dissensus, political deadlock, and eventually the purge of Mao's radical supporters. As part of a conclusion, Dittmer illuminates yet a fourth pivotal period: the de-Maoization and reform of 1977–81.

Yet this is not a political history. Dittmer is preoccupied with the overall *meaning* of these decades, reanalyzing each of these periods with an eye to his overall interpretive theme: that the Chinese party, up through the death of Mao, was struggling to "continue the revolution," while disagreeing about what this meant. "Continuing the revolution," of course, was the doctrinal justification for Mao's personal dictatorship and political repressions. From the outset, it is clear that Dittmer is preoccupied with what Mao was thinking, why he came to the judgments he did, how he and others reacted to the initial failures of his doctrine's implementation, and what ultimately were the political and social constraints that defeated his vision in the 1970s.

However Mao-centered this conception may be, Dittmer uses his own, not Mao's terms of analysis. Revolution requires, he argues, a charismatic leader who performs a "salvationary mission"; an "illegitimate authority structure" to be overthrown; and a "mobilizable mass constituency" to propel against this structure. Dittmer effectively illuminates the constraints placed on continuing revolution by China's changing circumstances, and how and why the leadership fell out over the issue. He also fashions a metatheoretical language to convey his arguments, for instance, *emergent* versus *residual structure, smashing frames, subjective* versus *objective charisma,* and *evacuation of mission.* While such language is initially difficult to unravel, it becomes less so with familiarity and serves effectively to integrate a wide range of material.

Integrating such a broad-ranging study is no small feat. Dittmer ranges widely across the Chinese scene: commenting insightfully on elite politics, the maneuverings of national and provincial bureaucratic constituencies, the building of factional networks, the evolution of Mao's thought, grass roots social organization, and the social origins of Red Guard factionalism. Especially valuable is Dittmer's extended analysis of the "late Cultural Revolution": from the end of the Red Guard movement in 1968 to the arrest of the "Gang of Four" in 1976. Without once resorting to simple narrative, Dittmer analyzes the evolving coalitions and conflicts among top party personalities, their factional networks from the capital to the provinces, and grass roots social conditions and political ferment, and provides a compelling account of why and how Cultural Revolution radicalism lost its appeal and support in the 1970s.

In such an ambitious work of interpretation, it is unfortunate that Dittmer does not engage other interpretations and arguments in scholarly dialogue. How does this interpretation differ from others? Indeed, to what long-standing questions about Chinese politics does it offer new answers? Moreover, how does the Chinese effort to continue the revolution compare with the way that the same problem has been handled in similar political regimes? How is this interpretation relevant to others' efforts to understand the generic features of de-

Stalinization, political mobilization, and the contest between forces of conservatism and reform? On these and other questions, which would help us to locate the study in a broader scholarly discourse, the author is largely silent.

ANDREW G. WALDER

Harvard University

Mozambique, a Dream Undone: The Political Economy of Democracy, 1975–1984. By Bertil Egerö (Uppsala, Sweden: Scandinavian Institute of African Studies, 1987. 230p. $44.50, cloth; $29.50, paper).

Mozambique: A Dream Undone belongs to an easily recognizable genre in African studies: the committed, partisan book, written by a disillusioned sympathizer of a revolution gone sour. The 1960s spawned such books on Algeria, the 1970s on Tanzania, and the 1980s are producing them on Mozambique. Such books have predictable strengths and weaknesses and this one is no exception. Egerö makes a serious attempt to understand what happened in Mozambique since independence, killing the dream of an egalitarian and flourishing society and leading to the present economic and military chaos. He starts from the assumption that socialism can work and in fact should work, and tries to explain why it did not in this case. The assumption is open to question, but it helps Egerö focus on what actually happened in Mozambique, and this should prove helpful to readers of any ideological persuasion. On the negative side, however, the book has a tendency to get lost in a theory of what socialism should be, setting impossibly high standards against which Mozambique is measured.

The result is a very uneven product, probably much more interesting to a reader already familiar with Mozambique than to somebody seeking a basic understanding of the country. The book contains adequate information, although it does not add much to what has been published elsewhere. Unfortunately, the information is poorly organized, unsystematic, and difficult to follow—the book needs some good copyediting. A patient reader could reconstruct a chronological overview of postindependence Mozambique from the information provided in the book but would have to work hard to do so.

The author is more interested in analyzing issues than in providing information and seems to forget that the two cannot be separated. The most interesting aspect of the book is the discussion of the role of the state in a country following an ideology of *poder popular,* which Egerö translates as "popular power," rather than the more commonly used, and more distinctly socialist, "people's power." Unfortunately, the discussion comes very early in the book, before even the most basic information is provided, and would thus be confusing—or meaningless—to most readers.

Egerö shows how the ideology embraced by Frelimo, before and after independence, called for people's power. What emerged was state power, the spreading of bureaucracy and centralization resulting in a top-down approach, "power by the popular government *for* the masses," rather than "power *by* the masses" (p. 35). Egerö predictably regrets this outcome. Less predictably, he also tries to explain it, rather than simply condemning it.

"In the post-liberation period, the party-state had before it a far more complex web of interrelations, international as well as internal, through which to develop a strategy for social *and* economic transformation. At the basis of colonial society was force; the relaxation of repression in the last few years had shown directly in for instance a decline in peasant cultivation of cotton" (p. 31).

The leadership was thus caught in a real bind. With a society used to force, and neither peasants nor workers ready to become the motor of revolution, "Frelimo had to rely on the state, the embodiment of its class enemy, to create the material conditions for advancement" (p. 31).

Mozambique: A Dream Undone is not the book on which to rely for an introduction to postindependence Mozambique. Joseph Hanlon's *Mozambique: The Revolution under Fire* (1984) contains much better and clearer information concerning the same period. Egerö also stops with 1984 and thus does not discuss the very important changes that have taken place since the signing of the Nkomati accord. For a reader seeking a better understanding of Mozambique and of the process of socialist transformation in Africa, the book offers, despite its shortcomings, food for thought.

MARINA OTTAWAY

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Political Change in Greece: Before and after the Colonels. Edited by Kevin Featherstone and Dimitrios K. Katsoudas (New York: St. Martin's, 1987. viii, 301p. $37.50).

The study of modern Greece has been largely ignored by social science scholars. Since it is not an advanced industrialized state like those of Western Europe, nor a socialist state in the Eastern bloc, nor for that matter part of the Middle East, it has been considered a small country of little interest or importance, except for its strategic role in the eastern Mediterranean. This lack of interest has been particularly pronounced since the end of the Greek civil war in 1950 and Greece's firm consolidation into the Western bloc as a client of the United States. However, Greece's entry into the European Community in 1981, followed by the entry of Spain and Portugal in 1986, has prompted studies both of the European Mediterranean as a region, peripheral both geographically and economically to the core of Europe, and of the individual states in this region. One consequence has been a revival of studies of the Greek policy and society.

Political Change in Greece is a much-needed volume that fills a void in the existing literature on Greece. Its primary purpose is to look at the continuities and discontinuities of the Greek political system prior to and after military rule, which lasted from 1967 to 1974. Consisting of 14 articles, two of which are revisions of previous publications, the volume's focus is on the informal political institutions of Greece. A brief historical introduction is followed by a discussion of the 1975 constitutional framework and of the prevailing public attitudes as the context for the articles that follow. The bulk of the material deals with political parties, interest groups, the media, and the military. Except for two articles, one by Van Coufoudakis dealing with Greece's foreign policy from the end of World War II until 1985 and one by Susannah Verney on Greece and the European Community, domestic politics and policies are not considered.

It is disappointing that this much-needed study, particularly on developments in the postjunta years, fails to provide the reader with a comprehensive understanding of the dynamics of Greek politics. Overall the articles are descriptive historical overviews lacking in analysis. Nor are the individual articles integrated within an overall framework or general theme that would enable the reader to place the discrete articles within a coherent context. As a consequence the volume appears disjointed, leaving unanswered questions as to the relevance or significance of the material presented. Nor is there a satisfactory explanation as to why the military warrants a separate article while other political institutions such as the executive, the legislature, and the judiciary are ignored. While it can be argued that the military has been critical for political developments in Greece, the role of these other institutions—in other words, the nature of the Greek state—has been, and is, of the utmost importance.

This volume would have been immeasurably strengthened if it had included material on the socioeconomic foundations of Greek society and its changing interconnections with the political spectrum. For example, a consideration of both the critical role the petty bourgeoisie has played by shifting party affiliation and therefore electoral outcomes and the roles of other class interests and of political style, would have added depth to the several articles on the Greek political parties. An exception to the volume's failure to provide an overall synthesized framework is Mouzelis's brief conclusion, an abbreviated version of an article that appeared in *Le Monde* in December 1985, addressing the issue of political change in Greece as it pertains to the functioning of the postjunta democratic polity. The issues he raises are significant. His balanced discussion of the persistence of the "personal-fragile" nature of the current Greek party system—in particular of the governing party Pan-Hellenic Socialist Movement (PASOK) despite some changes from the prejunta years—leads him to question the viability of civil society.

Other than Coufoudakis's interesting article on Greek foreign policy, in which he analyzes the efforts of the postjunta governments to enhance Greece's autonomy and independence and the constraints they face in implementation, none of the authors analyze the impact of historic foreign penetration on the functioning of domestic politics and institutions. It is difficult meaningfully to understand the political configuration in Greece while ignoring, as these articles do except for Veremis's on the military, the interventionist role of Greece's patrons and their penetration of the state structures and interest group organizations.

Within the parameters of this volume the quality of the individual articles is uneven. Among the best is Kapetanyannis's on the Communists, which, although impassioned, is an incisive analysis of the evolution of the Marxist Left in Greece. Although some of Featherstone's assumptions in his article regarding specifics on PASOK are dubious, his discussion of the party's ideology and its populism is highly informative. Fakiolas's article on interest groups highlights both the linkage between party affiliation and interest groups and their excessive legal regulation by the state. On the whole this volume does contain considerable information on the continuities and discontinuities in the Greek political system following regime change that should be of interest not only to those concerned with Greece but also to those involved with Mediterranean politics and with the results of democratic rule following dictatorial rule.

ADAMANTIA POLLIS

New School for Social Research

China's Second Revolution: Reform after Mao. By Harry Harding (Washington: Brookings Institution, 1987. xvii, 369p. $32.95, cloth; $12.95, paper).

Harry Harding offers a case-by-case study of post-Mao China's major political and economic reforms from their origins to early 1987. This book's strength is breadth and detail. As the most comprehensive study of reform in post-Mao China by a single author to date, it deserves attention.

The book is carefully organized in three sections. The first, on the origins of reform, sums up the failures of the Mao period, noting both economic inefficiency and the political "crisis of confidence." Harding argues that after Mao died change was inevitable, but the direction and degree of reform was largely determined by the reformers. He divides the reformers into moderates favoring incrementalism and radicals favoring fundamental reforms and attributes the cyclical pattern of reform and retrenchment to factional conflict.

The second section, on the content of reform, includes chapters on restructuring the economy, opening China to the world economy, liberalizing political life, and institutionalizing the post-Mao reforms. Harding is careful to balance achievements and problems, drawing careful lists of each before making conclusions. It is difficult to summarize his case-by-case analysis, but he argues that China's economy is headed toward neither capitalism nor orthodox socialism but toward some mixture of plan and market; that China's foreign trade will continue to grow, but China will not become a case of "export-led growth"; and that China is developing "consultative authoritarianism" but not even a "quasi-democratic" system. While Harding finds important improvements in China's political institutions, he is concerned that the reforms may not yet be sufficiently institutionalized to ensure continuity after Deng's departure.

The third section considers the future of reform and its impact on China's foreign policy. Harding finds China's foreign policy pragmatic and increasingly nationalistic. He argues that the United States will benefit from supporting China's modernization, but suggests that minor conflicts are inevitable. He considers future scenarios from a revival of radical Maoism to still more radical reform but argues that in all likelihood, the future will more or less resemble the present.

Harding's detailed empirical analysis is a strength, but he does not develop a systematic theoretical analysis. Some terms are used ambiguously. For example, he commends Deng's characterization of the reforms as "China's second revolution" (p. 1) but also states that "much of the basic structure of late Maoist China remains intact" (p. 4) and that "the core of the reform program . . . is limited liberalization" (p. 48). The "first revolution" was more than "limited" and left relatively few of the structures of Guomindang China intact. Despite labeling the political reforms "limited liberalization," elsewhere Harding argues that Deng does not have liberal values. This ambiguity leaves the reader unsure what significance Harding thinks the reforms have.

A more theoretical analysis might have developed the broader picture more fully and reached different conclusions about the prospects for reform. Harding views the reforms largely as a result of the will of the reformers. In the closest he comes to a structural argument, Harding considers the problems inherent in a semiplanned, semimarket system but argues that the reformers can choose to retreat or to push on. This argument makes an extremely important point: politics and not economics remain the root of Chinese society. But what range of options is really open to reformers? Harding's discussion of the Mao period gets the facts right but does not capture the real horror of Mao's campaigns. If Mao's legacy was a real crisis of state, then the reformers have no choice but radical reform. A comparative perspective would point out that reformers in different socialist countries have chosen similar reforms, which leads to the conclusion that reform strategies reflect structural problems of socialism. A more consistently structural perspective would conclude that the process of reform is also limited by state structures. Harding notes that "a veneer of unity, which masks a cauldron of conflict underneath, often characterizes Chinese politics" (p. 289) but does not examine the sources of conflict. If critical conflict is inherent in the structure of China's state, then the cyclical pattern of reform and retrenchment is not just a matter of conflicting viewpoints but reflects deeper dilemmas. Such dilemmas might lead to a radical departure in one direction or another, as has happened so many times in China's recent history.

This book is well researched and reasonable. Its conclusions will be supported by many China specialists. It is strongly recommended, especially for non-China specialists who want information on concrete issues and for policymakers or business people who want scholarly research without excessive jargon. China specialists will learn something from Harding's discussion of the strategies of moderate and radical reformers, and may find it a useful reference, especially considering the wealth of statistics. Harding deserves a wide readership.

BARRETT MCCORMICK

Marquette University

Marxism and the Crisis of Development in Prewar Japan. By Germaine A. Hoston (Princeton: Princeton University Press, 1986. xviii, 401p. $40.00, cloth; $15.00, paper).

Although Japan's Marxists have never enjoyed great political success, they have had a substantial impact in scholarly and journalistic circles, helping to shape the contours of political discourse even in the early years of the 1920s and 1930s when the radical left was a target of "thought control" and state suppression. This book describes the principal Marxist interpretations of modern Japan through an account of the interwar debate between the two leading schools of the Rono-ha (or Labor-Farmer faction) and the Koza-ha (associated with the Japan Communist party).

The author skillfully blends chronological and analytical frameworks in tracing the debate; the central chapters relate how Japanese Marxists viewed the course of capitalist development in Japan, the concept of the "Asiatic mode of production," the emperor system, and the character of Japanese agriculture. There are also several contextual chapters that will be useful to many scholars unconcerned with Japanese Marxist theory per se; for example, an early chapter on the Comintern's policies toward Japan will interest students of Soviet foreign policy, and a concluding chapter on postwar Japanese developments in Marxist thought will serve scholars of the contemporary party system by clarifying the theoretical geneology of today's leftist parties.

The author sets up each facet of the interwar Marxist debate by describing the Western and Soviet writings that served as starting points for the Japanese theorists, demonstrating in the process a real mastery of both well-known and obscure thinkers who were studied by the Japanese. The analysis then records in some detail the efforts of Japan's Marxists to apply imported theoretical constructs that rarely fit their country's experience without modifications. Japanese Marxism is presented as an example of how many non-Western Marxist schools have had to adjust European strains of Marxist theory to fit non-European patterns of development.

The author emphasizes throughout that although the classical Marxist texts and the

Comintern's occasional theses on Japan narrowed the boundaries of the debate, their application required considerable creativity and produced some significant novelties in Marxist thought. It is difficult to identify a major new theoretical concept born of early Marxist thought in Japan; an innovation of such magnitude was unlikely given that prewar Japanese thinkers wrote almost exclusively about Japanese capitalism and not about capitalism in general. But their debates did establish some important precedents for later developments in Marxist theory, especially regarding the relative autonomy of the state and the nature of the transition from feudalism to capitalism; and they also generated valuable historical research as each of the principal factions sought to bolster its point of view.

The book's descriptive account reflects careful, painstaking research, and it cannot be faulted but for two gaps. The first is the paucity of biographical background on the debaters themselves. The reader naturally gets interested in these people while absorbing their ideas; but although the debate itself is brought to life here, the same cannot be said of its participants. A second lacuna is the relative neglect of the period from the mid-1930s to the end of World War II. The "conversion" *(tenko)* of so many Marxist theorists imprisoned in this era must have been accompanied by some interesting theoretical reflections, especially on topics such as the relationship of nationalism to Marxism. A more careful treatment of this period could only have underscored the importance of the prewar Marxist debate, because the social analysis conducted by the Left made a profound impression on leading bureaucrats and civil intellectuals of the "renovationist Right," whose ideas become dominant in Japanese politics in the late 1930s. This linkage between leftist and rightist ideas merited closer attention, especially since Hoston did devote an entire chapter to Kamekichi Takahashi's much earlier Marxian apology for Japanese imperialism (penned in 1927).

While the main thrust of this book is to describe the interwar Marxist debate in Japan and to place it in a broader theoretical perspective, Hoston occasionally offers assessments of the validity of the arguments described; these assessments are not as well grounded as the rest of the book. Although the author herself is clearly not limited to a Marxist perspective, there is no serious consideration of non-Marxist interpretations or extensive marshaling of evidence to test the claims of Marxist intellectuals. For this reason, the author's rather consistently expressed preference for the social analysis of the prewar Koza-ha (which supported the Comintern's view that interwar Japan had not yet achieved a bourgeois-democratic revolution) over that of the Rono-ha (which saw Japan as a bourgeois-dominated society in this era) lacks a firm foundation. Nonetheless, evaluative statements of this type form a very small part of the book, and Hoston's preferences do not appear to have swayed her descriptive analysis.

Overall this book is a welcome addition that will be of service to many scholars. Japan specialists will find that it contributes much to the established literature on the history of Japan's leftist labor movements (none of which addresses prewar Marxist theory with anywhere near the skill and detail to be found here) and also that it complements the spate of other interesting books on modern Japanese political thought that have appeared recently (e.g., the work of Carol Gluck, Sharon Nolte, and Miles Fletcher). Students of comparative and especially third-world Marxism will be grateful for an analysis of the Japanese case that speaks directly and competently to broader trends in political theory. Indeed, this book is one among many encouraging signs that the relative isolation of Japanese studies from the mainstream of disciplinary scholarship is coming to an end.

GREGORY J. KASZA

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Opposition in Western Europe. Edited by Eva Kolinsky (New York: St. Martin's, 1987. x, 400p. $35.00).

Opposition is fundamental to the operation of democratic government. Strangely enough, however, relatively little political research has been focused primarily upon opposition. The obvious classic is Robert Dahl's *Political Oppositions in Western Democracies* published in 1966, although early theoretical treatments by other scholars such as Ionescu and Kirchheimer were influential. Santori, Duverger, and Beyme have more recently dealt with the matter within a more general context of political parties and party systems, but a comprehensive and systematic analysis of contemporary oppositions is needed.

This book is not that analysis. While it includes some rather perceptive examinations of oppositions within individual countries, it lacks a focus or structure to hold it together as a simple comprehensive or systematic study. The official introduction includes four pages that state the general problem, but there is no conclusion. Instead three papers are included by Peter Pulzer, Klaus von Beyme, and Gordon Smith that discuss concepts of opposition. These, however, appear to have been written independently of the individual country studies that follow and of each other. What we get then is a compendium of some thoughts about, and approaches to, the study of opposition. Even though this is a common fate for most edited volumes, it still leaves us without a systematic analysis of contemporary oppositions.

Peter Pulzer's general analysis is perhaps the most useful of the three included in the book. In a chapter entitled "Is There Life after Dahl?" he suggests a trend away from both polarized pluralism and consociationalism. In country after country, he says, there is more confidence in the system's ability to absorb peaceful alternation of governments and to tolerate policy opposition. Paradoxically, the exception is Britain. In much of Europe, however, new concerns have arisen and old loyalties have lessened. Citizen groups and single issue movements call into question not only the old political agenda but also the traditional ways for articulating opinion. Although new methods will come along for expressing it, opposition will go on. Gordon Smith also argues that institutional democracy in Parliament is running well; it is the sharp challenges by diverse minority groups that make the lives of governments difficult. The major problem remains one of translating minority views into some kind of majoritarian assent.

The country studies in the book tend to deal with the questions normally raised within the context of each particular country. David Denver, for example, enters the political debate in Britain over the nature of the opposition since its division and fragmentation in the elections of the 1970s and 1980s. The classic model of adversarial politics has been lost, he argues, in the highly fluid political situation. In France, on the other hand, Bryon Criddle sees a substantial growth of consensus. It may not be the Butskellism of the 1950s, but it is a long way from the French electoral rhetoric of the 1970s. In reviewing the history of opposition in Germany, William E. Paterson and Douglas Webber find a "preponderantly co-operative cast." There rhetoric belies reality, although the Greens have recently caused the Social Democratic Party to move away somewhat from its essentially cooperative position.

Geoffrey Pridham states that the Italian Communist party (PCI) in Italy is both transformative and accommodative. Its search for legitimacy and the need to come to terms with the Christian Democratic party (DC) drives the first; the second derives its history and identity as a party. Pridham expresses some doubt that it can break out of its traditional role of opposition. Ken Gladdish offers a very interesting view of what has happened to politics in the Netherlands since accommodation. The decline of confessionalism has made the center more attractive, not less. Although the electorate perceives itself as a divided society, there is a high level of consensus about how problems should be handled and how policy should be produced. Accommodation, in other words, has worked. Benny Pollack and Jean Grugel argue that Spain is moving toward a nationally based two-party system with some regional variation. The Left has succeeded in becoming a "natural party of democracy" because the Right remains tainted as a "natural party of authoritarianism."

One of the strengths of the book is the inclusion of a consideration of extraparliamentary oppositions. David B. Capitanchik examines the reduced role of the Trades Union Congress (TUC) as an effective political opposition in Britain and blames this on the decline of traditional industries and their work forces. Michalina Vaughan argues that the "wrong Right" is deeply rooted in French history but has shifted from those who reject the Republic totally to those who reject the standards on which consensus rests. Eva Kolinsky points out that legitimacy of German institutions is still questioned by pockets of opposition. A confrontationalist core does not yet allow parliamentary politics to speak fully to the new issues. Grant Amyot examines new political movements in Italy and finds that the decline of the traditional subcultures and ideologies weakens the old parties and leaves more room for new social movements. The parties, he believes, will have some difficulty getting the new demands together with existing interests so that the new movements will increasingly seek a voice inside as well as outside parliament.

The book is not an easy one to read. It has its share of turgid prose masquerading as insightful analysis, and the densely packed typeface is inexcusable. Even so, it is worth reading. It contains some very useful detail for students interested in European politics.

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Development Strategies and the Status of Women: A Comparative Study of the United States, Mexico, the Soviet Union, and Cuba. By Margaret E. Leahy (Boulder, CO: Lynne Rienner, 1986. 167p. $25.00).

Although not based on original research by the author, this brief and concise text offers a lucid distillation of virtually all available literature in English on the subject of women and the political economy of development in the United States, Mexico, the Soviet Union, and Cuba. While not providing new information, by analyzing and interpreting the available literature on the four countries in the context of liberal-capitalist and Marxist categories as different approaches to development, Leahy presents an engaging perspective that calls for a fundamental reexamination of the role of ideologies in promoting women's equality.

Unlike many "comparative" studies, this book provides throughout the text a genuine comparative analysis of the four countries and the two ideological models as they affect the status of women. After a solid review of the evidence and a penetrating critique of the contradictions and disparities between premises and results within each country and each ideology, the author makes a compelling case in favor of the Marxist model: "Although none of the nations studied has placed issues of women's equality at the top of its development agenda, those nations that have pursued a strategy for women's equality reflective of a Marxist logic have tended to do so to a greater extent than those that have followed a liberal capitalist logic. In many ways, the present formulations of capitalism and liberal democracy act to constrain progress toward women's equality" (p. 124).

Within the Marxist example, the juxtaposition of the Cuban and Soviet experience is particularly revealing. The Soviet Union has a longstanding commitment to women's equality; women have reached very high levels of participation in public affairs and have achieved considerable gains in terms of economic position, political power, and social status. Yet the achievements of Soviet women have resulted in their assuming a double burden: virtually exclusive responsibility for household maintenance on top of work outside the home. This is the result of unchanged traditional attitudes toward women's role in the home, reinforced by the failure of the state to develop adequate surrogate domestic services. However, given the logic of the Soviet system and its commitment to social equality, the author still concludes that with increasing material well-being, the country will move closer to genuine equality for women. The Cuban example indicates how more rapid progress becomes possible: while in the Soviet case further progress toward equality is conditioned by changes in the material condition of society, in Cuba the impediment of economic constraints has been minimized by a more active and pervasive governmental role in setting up comprehensive social legislation and in supporting political organizations that promote women's equality. And the public commitment has in turn influenced the socialization process. The lesson, of course, is that even within the Marxist logic—with its commitment to equality—a socialist political economy will not automatically engender equality for women unless accompanied by active governmental intervention and an explicit policy of linking women's equality with socialist transformation and economic progress, as happened in Cuba.

On the other hand, in a capitalist country (such as the U.S. or Mexico), the primacy of economic efficiency as the guide to production and profit easily results in conditions that mitigate against women's equality. It becomes costly and inefficient for private business to assume the burden of providing surrogate support services; and at the same time the role of the state is limited to that of arbiter among interest groups. Given the dominance of the capitalist logic, the resulting and prevailing political ideology in such countries is for a very circumscribed governmental role: promoting equal *civil* rights for women but not special benefits and rights capable of addressing women's special needs.

While cogent and persuasive in the context of the four countries examined here, the fundamental theme of the book is undermined by the absence of any references to other examples of liberal-capitalist countries where the status of women in fact appears to be quite different. Based on Leahy's argument, Scandinavian and West European countries should exhibit a situation similar to that of Mexico and the U.S. Thus the demonstrable differences would seem to indicate that Leahy's argument notwithstanding, liberal-capitalist societies may in fact not necessarily present "an ideological barrier . . . inhibiting conscious attempts to change social ethos" toward women's equality (p. 127). Similarly, although the Cuban and Soviet examples obviously do illustrate important aspects of growth and development along the Marxist (or more correctly, the Soviet Marxist-Leninist) pattern, can we in fact state that these two countries typify Marxism as a "development strategy" in general and on the issue of women's status in particular? In the absence of an expanded comparative analysis to encompass a greater variety of Marxist and liberal-capitalist countries, Leahy's conclusions can at best provide preliminary material for further investigation and a stimulating debate—but, in all probability, they will not be able to gain acceptance as valid empirical theorizing.

PRIMO VANNICELLI

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Changing Boundaries of the Political: Essays on the Evolving Balance between the State and Society, Public and Private, in Europe. Edited by Charles S. Maier (New York: Cambridge University Press, 1987. ix, 417p. $49.50, cloth; $14.95, paper).

This volume of stimulating essays, edited by Harvard historian Charles S. Maier, grew out of meetings of the Joint Committee on Western Europe of the Social Science Research Council and the American Council of Learned Societies. Half of the 12 contributors are sociologists, a point that may have lent a certain new-discovery breathlessness to topics long considered by political scientists. Three are political scientists (Suzanne Berger and Ellen Immergut of MIT and Miles Kahler of the University of California, San Diego), two are historians, and one an economist.

Some of the essays are brilliant, but the collection as a whole offers no single subject or thesis. One is hard put to draw connecting links between them. Maier asks what is political; and the answer seems to be—potentially at least—just about everything, a finding few political scientists would quarrel with.

To be sure, specific historical, social, economic, and even political pressures turn the private into the public or the technical into the political. Occasionally, too, things go the other way as issues recede and are depoliticized. There is no clear-cut unilinear trend toward everything becoming political, much to the relief of some of the contributors.

Some of the essays do connect thematically. Alessandro Pizzorno, taking a leaf from certain historians, argues that "absolute politics" owes its genesis to investiture conflict of the eleventh century, in which Pope Gregory's efforts to expand the powers of the papacy triggered contrary kingly claims. The newly emerging kingdoms took over some of the absolute qualities heretofore associated with Catholicism. Mutatis mutandis, Suzanne Berger finds that the breakup and political failure of French Catholic "integralism" left its adherents to carry their passions into the labor movement and nonconfessional parties. What the church claimed in the eleventh century its descendants brought with them into the politics and unions of the twentieth.

Two British-focused essays also connect with each other, one on the development of the welfare system, the other on the politicization of health care. Massimo Paci finds the shift in welfare provision from charity and market to state-run administrations may have crested. With the return of private insurance we may be seeing a "revenge of the market." Paul Starr and Ellen Immergut, in perhaps the book's best essay, emphasize that there has been pressure to move medical care not only from the private to the public but also from the political to the technical. An area designated as "technical" may thus be removed from the political agenda and placed in the hands of professionals. This is only a momentary respite, though, for a crisis of costs soon grows out of the expansion of benefits and the budgetary autonomy of the doctors and administrators. The doctors got their autonomy as part of the original bribe to get them to go along with the state health system.

Implicit in the essays is the question, Are we on the brink of major political change? Have postwar welfare states reached a logical conclusion in overextension and ungovernability? Some of the essays think so. Claus Offe believes the Greens have turned their backs on the growth-cum-welfare state and evolved an attractive new paradigm of quality of life, authenticity, and participation. Laura Balbo, a Milan sociologist and deputy in the Italian parliament, suggests new roles for women in the postwelfare state.

Probably the main thrust of the essays, however, suggests more of the same: political battles within the pluralist welfare state. Dutch economist Jan Pen sees no clear end to the expansion of transfer payments, and John Goldthorpe suspects Thatcherite neoliberalism will ultimately fail amidst heightened class tensions. Miles Kahler likewise sees no escape for Europe through supranationalism.

The essays suggest enticing comparisons with the United States and Japan, unfortunately left unexplored. Examining Western Europe alone no longer suffices; we should increasingly be looking at the first world as a whole. *Changing Boundaries of the Political* is worthwhile reading for Europeanists, more for the thoughts it provokes than for any answers it gives.

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Politics and Petroleum in Ecuador. By John D. Martz (New Brunswick, NJ: Transaction Books, 1987. xii, 432p. $34.95).

Considerable disagreement exists among students of Latin American politics about the impact of governing arrangements on public policy. While some contend that differences between authoritarian and competitive regimes are important for understanding the policy-making process and its outcome, others totally dismiss the policy relevance of regime type.

Politics and Petroleum in Ecuador makes an important contribution to this ongoing theoretical debate. It does so on the basis of a detailed analysis of the evolution of petroleum policy in Ecuador between 1972 and 1984. During this period the oil industry emerged as the driving force in the Ecuadoran economy, displacing export-oriented agriculture to become the nation's principal source of foreign exchange. The rapidity with which this development occurred was extraordinary. Whereas in 1971 oil had accounted for less than 1% of the nation's total foreign exchange earnings, by 1973 it was the leading export product.

John Martz traces the way in which four successive Ecuadoran governments responded to the oil bonanza. The analysis is divided into two major sections. The first examines the two military governments of the 1972–79 period, when a nationalist and statist approach to the oil industry was initiated. The second major portion of the study focuses on petroleum policy under elected administrations during the 1979–84 period, when policy shifted away from the nationalist approach of the military. But it is not the policy contrasts between military authoritarianism and democratic pluralism that Martz emphasizes. In a cautious and carefully crafted analysis, he argues that regime differences had little impact on Ecuadoran oil policy. The retreat from the economic nationalism of the early boom years began under the military and reflected changing economic conditions rather than regime change. As a small producer incapable of influencing international oil markets but highly dependent on oil exports, Ecuador's vulnerability to changing external forces was pronounced. Hence Martz asserts, "The foreign debt, declining reserves, stagnating exports, and a sagging international petroleum market all combined to shift policy emphasis from reformism to an embattled struggle to maintain the recent status quo. . . . Such a dramatic turnabout was largely independent of the nature of the regime" (p. 390). Martz also shows that basic policy orientations and bureaucratic styles remained remarkably constant across the 12-year period. Economic pragmatism held sway throughout, as did a technocratic approach to the management of the oil industry. Hence Martz concludes that "where petroleum policymaking is concerned . . . the conclusion is inescapable that the nature of the regime was not a major determinant" (p. 393).

The obvious question about a study of this type is its broader relevance. As Martz implicitly recognizes in his carefully qualified conclusions, generalizing from a study of a single policy area in a single country is hazardous, especially if there is reason to think that the policy area and country in question are atypical in some way. Would a study of policy in an area of state activity less directly affected by international economic forces than petroleum have led to a different conclusion regarding the impact of regime differences? What about a study of petroleum policy in a country where regime differences have been more pronounced than in Ecuador? In emphasizing the role played by international economic forces, Martz suggests that the answer to the first question may be *yes.* Yet the possibilities of innovation in all areas of policy were conditioned by external forces due to impact of export revenues on government budgets and the economy as a whole. Ecuador is hardly unique in this regard. Hence Martz's findings, which underline the severe constraints under which decision makers operate in small Latin American countries, are clearly of broader theoretical relevance. With respect to the second question concerning the possibly atypical nature of Ecuadoran politics, Martz recognizes that the differences between military and elected regimes in Ecuador have been rather formalistic due to the strength of elitist influences and the limited nature of popular participation in public affairs. He thereby offers a major caveat to his conclusions, hinting that the alternation in power of inclusionary democracy and exclusionary authoritarianism may produce dramatic policy changes even if shifts between *dictablanda* ("soft" dictatorship) and oligarchical democracy do not.

In addition to its contributions to the study of Latin American policy making, Martz's book represents a major addition to the limited body of literature on modern Ecuadoran politics. It provides a good overview of events between 1972 and 1984, including the Ecuadoran military's seizure of power and the subsequent process of democratization. Students of bargaining relationships between multinational corporations and host countries will also find this an interesting volume.

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Authority and Inequality under Capitalism and Socialism: USA, USSR, and China. By Barrington Moore, Jr. (Oxford: Oxford University Press, 1987. viii, 142p. $28.00).

In 1966 Barrington Moore, Jr., concluded his classic work, *Social Origins of Dictatorship and Democracy,* with a pessimistic prognosis for the future of capitalist democracy as a model for modernizing societies. He wrote then that democratic capitalism, "is an historical formation that . . . is now past its zenith. *If* the revolutionary wave continues to sweep through the backward world in the years to come, that is scarcely the form it is likely to take" (p. 483). More than twenty years later, in his latest book, the doyen of comparative historical sociology is equally pessimistic about authoritarian socialism as a rival model to democratic capitalism. He concludes the current book by arguing that neither model is capable of resolving problems of authority and inequality. If the reasoning is sound, it is a profoundly depressing message for students of politics as well as for leaders of third-world revolutionary movements. Unfortunately, upon examination this reviewer finds that the reasoning is indeed sound and the implications are devastating; we seem to have run out of alternatives to resolve some of the great problems that afflict developmental revolutions.

Where one might take issue with Moore is not with his conclusions but with the extent to which they are widely accepted among revolutionary leaders in the third world. He says that the present time is exceptional in that it is the only epoch in which there are no credible models deserving of emulation. While at various points in history Athens, Confucian China, England, France, the United States and, most recently, the Soviet Union and China have served as ideals worthy of copying, today, "the big models have been discredited" (p. 116). Perhaps they have in some quarters; but revolutionaries in Central and South America still seem largely committed to socialist models, and the upheavals in Southeast Asia have led in the same direction. Indeed, the Khmer Rouge in Cambodia and the Sendero Luminoso in Peru are recent examples of unmitigated adherence to an extreme form of state socialism.

But perhaps these are exceptions that only prove the rule that Moore is attempting to formulate in this persuasive collection of essays, a revised and expanded version of a series of lectures that he gave at Oxford University in 1985. In this volume he uses the cases of the United States, the Soviet Union, and China to compare key differences between capitalism and socialism. Specifically, he focuses on systems of authority and inequality interpreted as the principal mechanisms employed by complex societies for placing individuals at different positions in the social hierarchy and for determining how well or how badly each position is to be treated by the society.

Moore's review of the evidence, as always, is presented in a provocative and well-documented fashion. It leads him to a set of generalizations that finds that capitalism and socialism have far more in common than different in their systems of authority. Both capitalism and socialism share a dislike for authority, a disbelief that different social groups will inevitably have conflicts of interest, and a refusal to confront the overwhelming evidence that systems of authority and social group conflict invariably lead to benefits for some groups that come at the expense of other groups.

Moore also finds a great deal of similarity in socialist and capitalist systems of inequality, even though each one each departs from quite different initial principles. Socialism attempts to develop the "new socialist man," one who will be motivated by nonmaterial incentives in a classless society. Capitalism relies upon individual greed for motivation and the guiding hand of the market to transform individual gains to societal gains. But in the most important socialist systems, the Soviet Union and China, market forces are being increasingly used to supply the missing motivation needed to rescue the economy from complete stagnation. Capitalism, for its part, became increasingly concerned over the failure of the market to provide benefits for the poor and substituted increasing doses of government regulation and larger and larger income transfers to those excluded from the prosperity that was brought on during the period of rapid industrialization. While it is true that the capitalist world of the 1980s is increasingly resorting to a return to the market via privatization of state-controlled enterprises, Moore does not believe that the reemergence of the market in either socialism or capitalism will resolve the underlying problems that face both systems.

Capitalism, Moore shows, remains plagued by unemployment, inequality of wealth and income, and irresistible urges to exploit less powerful nations via imperialistic actions. Socialism, for its part, has failed universally to produce material abundance anything like that common to the leading capitalist societies, while at the same time oppressive socialist bureaucracies have severely restricted freedom of choice and brutal secret police forces have constrained freedom of action and thought. To these failures of socialism, Moore adds their inability to reduce to any significant degree the inequalities characteristic of capitalist societies. Although not addressing directly the evidence marshaled by those who have conducted empirical tests of world system theory showing that income inequality in socialist systems is far lower than that in capitalist systems, Moore argues convincingly that the party elite enjoys material rewards (luxurious housing, excellent food, special health care, and so on) as well as other perquisites of power and influence that make socialism and capitalism virtually indistinguishable. He goes further by arguing that the supposed triumphs of socialism in eliminating poverty are also unsubstantiated, with the socialist poor "making do at the margin of subsistence" (p. 118). While poverty has not been eliminated in the capitalist United States either, the continued rise in living standards there means that those defined as the poor enjoy, from both comparative and historical perspectives, an astonishing abundance of goods and services.

Moore concludes this study by arguing that neither socialism nor capitalism will eliminate authority or inequality. Authority in the form of bureaucracy is needed by socialist systems to replace the market function, while it is required in capitalist societies to allocate welfare benefits and protect against the abuses of big industry. Inequality, for its part, is essential to stimulate (rather than coerce) people to work so that goods may be produced to satisfy demands. In light of those conclusions and his analysis of the failure of either model to solve our major problems, Moore argues against those who have turned to religious fundamentalism as the only remaining way out. He suggests instead that we ignore appeals for "politically glamorous" solutions while we encourage our leaders to seek feasible, as opposed to millenarian, dreams.

This is a little book but it is far from a small one. It reflects the distillation of a great deal of thinking about big issues. As such it would be an ideal volume for a wide range of courses on the politics, sociology, or economics of development. One can only hope that Oxford University Press will soon publish a paperback edition to facilitate its use in the classroom.

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Transitions from Authoritarian Rule/Prospects for Democracy. 4 vols. Edited by G. A. O'Donnell, P. C. Schmitter, and L. Whitehead (Baltimore: Johns Hopkins University Press, 1986. xxxix, 710p. $49.50, cloth; $9.31 ea., vols. 1-3; $5.91, vol. 4).

The four edited volumes of essays that make up *Transitions from Authoritarianism/Prospects for Democracy* are the product of an ambitious cross-national research project sponsored by the Latin American Program of the Woodrow Wilson Center of the Smithsonian Institution in 1979. The collection encompasses 13 country studies (5 in Europe, 8 in Latin America); 7 theoretical essays exploring the military, economic, international, and methodological aspects involved in the study of authoritarian regime transitions; and a concluding reflection on the entire enterprise. Because of its scope and theoretical depth, this is the best examination of the subject to date and as such constitutes a major addition to both the literature on modern authoritarianism and that of comparative politics in general. Since space constraints preclude the type of detailed review that a work such as this deserves, attention here will concentrate on highlighting key points and the general theoretical argument that underlies the whole project rather than on the case studies and individual essays. The reader is nevertheless advised to take the time to dwell on the country studies and conceptual chapters at length, as each offers a rich and varied panorama of the factors involved in freeing national political landscapes from authoritarian domination. Suffice at this point to note that all of the volumes are uniformly well written (although in some cases a bit dated), and several essays (such as those of M. A. Garretón on Chile and A. Przeworski on problems involved in the study of transitions) are simply brilliant.

Transitions from Authoritarianism completes a trilogy of modern studies on the dynamics involved in authoritarian regime emergence, maintenance, and demise that includes landmark works by Linz and Stepan (1978), O'Donnell (1973, 1982), Collier (1979), Malloy (1977), and Stepan (1970, 1979), a voluminous periodical literature, and dozens of monographical works of a related nature. In addition, in assessing the prospects for democratization in the postauthoritarian context, the participants in this project follow lines of inquiry previously developed by the likes of Rostow, Dahl, and Gramsci, to name just a few prominent others. As a result, the collection not only serves to complete the trilogy of studies on modern authoritarianism, it also develops the logical basis for future studies of democratic consolidation in countries emerging from authoritarian rule. It is therefore a landmark work in the full sense of the word, as it bridges the gap between two previously distinct literatures and opens the way for an entirely new field of study.

That some of those mentioned served as guiding forces of the Wilson Center Project should come as no surprise, as it represented that natural extension of ongoing work. Moreover, by combining forces and expanding the scope of inquiry across time, regions, and disciplines, the editors hoped to sharpen and expand the focus of discussion about the evolutionary dynamics of modern authoritarianism beyond the tired structuralist-behavioralist debates. Rather than the more "static" pictures normally associated with the study of stable political regimes, here the concern focuses on the dynamic interplay of structural and superstructural variables in the transition from authoritarianism, and in particular on the types of strategic interaction between various sociopolitical agents that lead to successful transitions. The highly fluid, reversible, and thus inherently uncertain, nature of this process requires that structural conditions be treated as general parameters or constraints rather than as determinants of action, within which events such as strategic decisions, actor miscalculations, and unanticipated sectoral responses all respond to, and then combine to form, a wide range of intervening factors in the evolution of specific transitional "games." As O'Donnell and Schmitter point out in volume 4, "unexpected events (fortuna), insufficient information, plasticity, and even indefinition of political identities, as well as the talents of specific individuals (virtu) are frequently decisive in determining outcomes. This is not to say that macrostructural factors are not still 'there' . . . but even those mediations are looser, and their impacts more indeterminate, than in normal conditions" (p. 5). Hence an underlying thread throughout this work is the acceptance of the type of behavioralist propositions often associated with Juan Linz, in which individual decisions and ongoing strategic interaction do matter even if generally constrained by objective structural conditions. The very fact that several countries sharing roughly similar "macro" traits (such as dependent capitalism) evolved different transitional "paths" (to use a term utilized by Stepan in volume 3) attests to the validity of such an approach. Structural conditions are important, to be sure, but analysis of their impact must be overlapped with conjunctural and other superstructural factors that broaden the range of indeterminacy with regards to possible outcomes. This perspective serves as a common starting point for all of the essays in the collection, and is most evident in volume 3, in which the chapters' focus sequentially narrows from international variables to the two most important actors in these transitions, military and business elites.

Another common point of agreement is that the transition away from authoritarianism is most often an internal process, in terms both of its initiation and its subsequent development. As Whitehead and Stepan show in volume 3, a wide variety of external factors (ideological, economic, military, etc.) and actors (government and nongovernment agencies, international organizations, private interests, etc.) all play a role in facilitating different types of transition, but in most cases they are not the determinate factors. Only in West Germany was democracy directly imposed by an external agent (in the form of the Allied forces of occupation), and external events such as defeat in war at best serve as precipitating events in the collapse of authoritarianism (as the examples of France and Italy in 1943–44, Greece in 1974, Portugal in 1974, and Argentina in 1982 suggest). In each case, internal tensions within the authoritarian coalition and rising domestic opposition had already manifested themselves to the extent that external factors merely served as catalysts for change. The point is that while certainly important, more often than not external actors and events may contribute but cannot of themselves bring down an authoritarian regime; that is the province of domestic factors and agents.

Another key issue is the differentiation between authoritarian regime liberalization and democratization. As Przeworski notes, regime change begins the moment elements of the dominant coalition break rank and seek external sources of support among previously excluded sectors of society. Liberalization refers to a gradual "softening" of authoritarian rule, characterized by the incremental extension of civil liberties and individual and sectoral rights of political voice and participation. However, regime change does not always entail liberalization and can in fact merely imply movement to other, often harsher forms of authoritarianism. This process, and the possibilities for liberalization, depend on the strategic dynamics within and between the regime and opposition. In particular, they depend on the interplay between authoritarian hardliners *(duros)* and softliners *(blandos)* on the one hand, and their evolving relations with moderate and maximalist sectors of the opposition on the other. Where *duros* or maximalists prevail, the chances of liberalization are slim.

Where *blandos* or moderates outweigh extremists on each side, the prospects for liberalization improve. It should be underscored that actors do not remain locked in any single position and can in fact move from one to another depending on circumstances. The point is that for a variety of reasons (economic success or failure, achievement of security or political objectives, quest for legitimacy or expanded bases of support) the dominant coalition begins to erode, and authoritarian actors eventually look to civil society for alternative bases of support. Depending on the character and objectives of those who respond to this "opening," authoritarian liberalization may or may not occur.

Democratization refers to the expansion of civil society, reflected in the posing of previously excluded group demands upon the authoritarian incumbents, in the reforging of political identities, and in the horizontal expansion of social networks comprised of both traditional and newly aggregated collective agents. This "resurrection of civil society" (to use O'Donnell's phrase) may or may not parallel authoritarian liberalization, and in fact can either follow or precede it. The latter situation can be considered a "bottom-up" process of transition, and is most often evident in cases of authoritarian collapse, as Italy, Portugal, and, most recently, Argentina suggest. On the other hand, where democratization follows or parallels liberalization, this can be considered a "top-down" process of transition in which authoritarian power is incrementally relinquished or devolved rather than surrendered or lost. Post-Franco Spain and Brazil provide the most often cited examples of this type of transition.

There is a major irony involved in the move toward democratization, because the need to mollify authoritarian (particularly hardliner) concerns about the postauthoritarian context leads the democratic opposition to move beyond mere moderation and accept limits on the extent to which democratization can occur. More specifically, this involves limiting democratization to the procedural or formal level (party competition, open elections, etc.), and avoiding rapid movement toward more substantive democratization at the societal and economic levels. Moreover, it also often involves ethical compromises when addressing the "legacy" problem caused by military violations of civil rights and other transgressions committed during their tenure in power. This in turn also involves the extent of militarization of the authoritarian regime, as Rouquié notes in volume 3. Only in Greece and Argentina were any officers brought to trial for crimes committed during their rule, and in each case prosecution was limited to the leaders of the regime itself, not their subordinates. In fact, de facto amnesty has been granted the military qua institution in Argentina, Brazil, and Uruguay, three countries in which human rights violations were most extensive, in order to lessen the possibility of a military-led authoritarian regression. As Garretón underscores, this could well be the price paid for freedom in Chile in the future. The basic issue involved here is that of extending certain sectoral guarantees to actors that would otherwise be disposed to accept the continuation of authoritarianism, in an effort to ensure the transfer of power and prevent an authoritarian regression in the near future. The result may be simply the substitution of a "soft" dictatorship *(dictablanda)* with a "hard" or "limited" democracy *(democradura)* that places serious constraints on sectoral participation and efforts at substantive change. That this poses fundamental problems for democratic consolidation should be obvious.

Another important influence on the character of the transition undertaken is the degree of institutionalization of the authoritarian regime. Long-lived, well-institutionalized regimes such as those in Mexico, Spain, and Brazil leave organizational legacies that shape the content of the transition process in a way that is not evident in less institutionalized authoritarian regimes. Ultimately, though, the transition from authoritarianism hinges on the actions of two social actors that constitute the major "swing" votes in such processes: the bourgeoisie, particularly its transnationally connected upper fractions, and the military hierarchy. Only when these actors decide, for reasons of expediency, self-preservation, or altruism, to support efforts to liberalize, does the path towards democratization become feasible. Until that point, the prognosis for a successful transition to genuinely open forms of government remains uncertain at best. It is here that the issue of political "pacts" becomes important, since it is "horizontal" elite agreements that serve as the vehicles through which mutual sectoral guarantees are tendered and the terms of the ensuing political competition are fixed. In fact, evidence suggests that "pacted" democracies such as those of Spain, Italy, and Venezuela are the most viable democratic forms to emerge in the postwar era, given the levels of social mobilization and organizational complexity that now characterize many late twentieth-century societies. However, subsequent democratic consolidation may well depend on the degree to which these initial elite pacts are vertically "deepened" by incorporating agents of previously excluded subordinate groups, something that rests on material, as well as nonmaterial, conditions of a conjunctural and structural nature (such as foreign debt or ideological conflict).

In summarizing the project's findings, O'Donnell and Schmitter use the analogy of a multilayered chess game to characterize these types of transition. Though a bit overdrawn, the analogy well captures the multiple and intertwined levels of analysis, varied types of strategic interaction, and myriad uncertainties that are integral components of such processes. In all cases, the outcome is very uncertain almost to the end of the "game," is extremely susceptible to reversal, and hence is totally contingent on the sequential outcome of conflicts lacking clearly established rules. However, one area that is not covered in their analysis is the role of learning processes in these transitions. After all, even chess players learn from past experience in order to improve their chances in the future. Thus it would have been worthwhile considering the role of learning processes in both civil society and regime incumbents in the move away from authoritarianism. The waves of liberalization-democratization that swept southern Europe in the 1970s were followed by a similar wave in South America in the 1980s. Did not regime incumbents as well as opposition groups in the latter look to the European experience for lessons on how to go about negotiating with each other? The steady references to "social pacts" on the one hand and to the avoidance of "another Nuremberg" on the other, suggest that this might in fact have been so. Did not the timing of the Brazilian *abertura* in 1974 have some relation to what was occurring at that time in southern Europe? Certainly Pinochet in Chile has taken heed of the recent events in neighboring countries, as have his opponents (in fact, some have suggested that Pinochet has abandoned the original Brazil-inspired model of gradual liberalization in favor of a Francoist vision of long-term personalist rule, something which may or may not give comfort to his opponents). In any event, this is one issue that remained absent from the collection, which, given its other attributes, is a small but surprising omission.

In summary, and recognizing the very superficial treatment awarded it here, this is a collection of essays that will serve as major elements in the study of comparative politics for years to come. Rich in detail, subtle in analysis, nuanced in presentation, and extensive in both its theoretical grounding and comparative scope, this is a must read for any serious student of the discipline.

PAUL G. BUCHANAN

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Asian Political Institutionalization. Edited by Robert A. Scalapino, Seizaburo Sato, and Jusuf Wanandi (Berkeley: Institute of East Asian Studies, University of California, 1986. x, 312p. $20.00).

This is the second of a three-volume study of the major issues confronting the peoples of Asia. The editors initiated the project in 1983 with three workshops focusing on economic development, political institutionalization, and security problems. After the customary revision and a second meeting in 1984, the conference papers were published in the Research Papers and Policy Studies series of the Institute of East Asian Studies at Berkeley.

The book opens with an introduction by Scalapino, who defines political institutionalization as "the process whereby a political structure is made operational in accordance with stipulated rules and procedures, enabling regularized, hence predictable, patterns of political behavior." Under ideal circumstances, institutionalization discourages personalized rule and "makes orderly, evolutionary change possible" (p. 1). It is clear that institutionalization, as defined here, promotes political stability and development. In this regard, Scalapino's views closely parallel those of Samuel Huntington.

The U.S. editor's conceptual framework is largely in accord with the theoretical assumptions of the authors of the 13 chapters in this book. Their primary emphasis, however, is not on formulating theoretical expositions. They describe, explain, analyze, and forecast regarding the party system, the bureaucracy, the military, and the leadership style in the country or area of their expertise. C. Chung traces, with a critical overtone, the development of "monocracy" in North Korea, and D. Pike dwells extensively on the failings of the existing party structure in Vietnam. Scalapino presents a comparative analysis of "Asian Marxist-Leninist Systems" in North Korea, China, and Vietnam and foresees the rise of technocrats and decentralization. Sato's interpretive essay on Japan's party system covers the last century and warns that institutionalization does not mean adoption of Western-type democracy. S. Han analyzes the "bureaucratic/authoritarian system" of South Korea and hopes for an eventual liberalization, albeit within a basically authoritarian context. D. K. Emerson emphasizes the role of the state in Southeast Asia and singles out adaptability as the most useful variable in discussing institutionalization. T. Shiraishi notes the divergent patterns of the praetorian military in Thailand, Burma, and Indonesia. A positive appraisal of Soeharto's efforts for evolutionary political development in Indonesia, the "New Order," is offered by Wanandi. L. B. Hiok's focus is on the People's Action Party in Singapore, while Z. H. Ahmad explains the reasons for evolutionary stability in Malaysia. The succession of military coups in Thailand since 1932 is described by C. Samudavanija who optimistically notes the stabilizing influence of "social institutions" such as monarchy, religion, and so on. C. G. Hernandez contrasts, in a tone tinged with despair, Marcos's rule with the tradition of civilian supremacy in the Philippines. In the last chapter, M. Weiner examines the contrasting patterns of institution building in India, Pakistan, and Bangladesh.

The preceding one-sentence summaries of these chapters fail to do justice to the wealth of information and insights that only area specialists can offer. A student of Asian politics or international affairs will greatly benefit by reading them. On the other hand, if one were to look for an extensive theoretical exposition on political institutionalization in Asia, this book is of limited value despite occasional intriguing and even provocative passages found in the chapters by Scalapino and Emerson, among others. Does the concept of institutionalization coalesce with, or is it a current version of, "modernization" and "development" of the 1960s? Is institutionalization desirable for all political structures? The title of this book whets one's intellectual appetite, to leave it only partially satisfied.

HAN-KYO KIM

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The South African Quagmire: In Search of a Peaceful Path to Democratic Pluralism. Edited by S. Prakash Sethi (Cambridge: MA: Ballinger, 1987. xvi, 512p. $29.25).

Power-Sharing in South Africa. By Arend Lijphart. (Berkeley: Institute of International Studies, 1985. x, 179p. $10.00).

Chronic political crisis in South Africa produces a steady stream of prescriptive Western publications that argue the imperative and feasibility of a democratic solution to that country's racial tragedy. This literature offers negotiational and constitutional formulas for the construction of a peaceable and prosperous policy. It is sometimes strikingly presumptuous, as for example the much-publicized manifesto, *South Africa: The Solution,* in which Leon Louw and Frances Kendall argue for the adoption of a South African version of the Swiss canton system. It reflects a penchant for problem solving by outside observers concerned that escalating violence and repression may lead to even greater tragedy. Often suffused with wishful thinking, it tends to underestimate the magnitude of political and racial polarization in South Africa and imposes formulaic rationality on the volatile reality of social cleavage, coercive rule, and frustrated aspirations.

The two volumes under review feature a shared commitment to values of democratic pluralism. But they are not prescriptive manifestos. Prakash Sethi of Baruch College's Center of Management has edited a collection of 37 mostly short (5-to-10-page) essays that stress the centrality of economic forces, notably business, to the dynamic of social and political reform. In his overview chapter, Sethi sets forth his own perspective, arguing that foreign divestment, disinvestment, and trade sanctions can only serve to disable, not empower black South Africans. A series of general essays by figures like Senator Richard Lugar, Congressman Howard Wolpe, and Archbishop Desmond Tutu, interesting because of the authors' political prominence, is followed by a plethora of pieces arguing the pros and cons of economic sanctions. Unfortunately, as Sethi notes, spokespersons for the African National Congress, labor unions, and other black movements did not contribute to the volume.

Some of the *South African Quagmire* essays bring new insights to bear, for example the contrasting assessments concerning compliance by U.S. companies with the Sullivan Principles. Cumulatively, the essays are informative and surprisingly free from repetition. But most, while well-reasoned and clearly written, are too brief to be substantively or analytically rich.

The *Quagmire* volume is nowhere more weighted than in the chapters dealing with education. The government position is cogently presented by Minister of [Black] Education and Development Gerrit Viljoen. It is not matched by a contribution from educational leaders or analysts associated with or knowledgeable about the many black-led educational organizations establishing educational agendas, running scholarship programs, conducting bridging courses, organizing correspondence education, and otherwise challenging the existing order in a sector that is key to black advancement.

Handicapping the volume's utility for ongoing discussion of Western policy and South Africa's future is the fact that divestment, disinvestment, and sanctions measures taken during the past several years have sharply undercut the proinvestment option. It is now probably too late to test the hypothesis that more socially engaged investment would effectively promote social justice.

Arend Lijphart's *Power-Sharing in South Africa* takes as its point of departure that the "gravity and urgency of the South African problem" that have given rise to "a host of proposed solutions" would be best addressed through consociational politics. He rejects majoritarianism, as well as undemocratic or partition proposals as undesirable, spelling out his reasons. He then defines and refines the concept of consociational democracy, taking pains to clarify what is and is not required to make such a decentralized system function and hold together a highly polarized society. He systematically addresses criticisms of consociational theory and argues that despite the violence and coercion in South Africa it is not unrealistic to advocate such a system. This is so, he states, because South Africa's "segmented" leadership will come to realize that "competitive behavior" is leading to conflict and disastrous consequences that reason dictates they avoid.

Power-Sharing sets forth consociational democracy as the only "possible" democratic solution for South Africa, while acknowledging that the "longer it is delayed, the more polarized the political atmosphere will become, the smaller the chances of setting up a successful consociation will be, and the more likely a violent confrontation will be." The importance of the Lijphart study does not depend upon the receding prospects of such implementation, however. It does not depend upon the triumph of consociational reason in South Africa. The heart of the monograph (chaps. 3–5) presents a definitive and clear definition of what is and is not essential to consociation. It stands as a theoretical and prescriptive statement of what a consociational approach to the resolution of communal conflict and polarization must entail. It will remain an invaluable reference for all those interested in consociation, whatever happens in South Africa.

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Understanding Political Development. Edited by Myron Weiner and Samuel P. Huntington (Boston: Little, Brown, 1987. 514p. $15.25, paper).

Political scientists have queried what happened to the mainstream developmental theory that so influenced comparative politics during the 1960s and early 1970s. A response is contained in this anthology of essays, which assesses work on developmental theory over the past two decades. This volume is organized into four sections.

A first part focuses on the third world, with a general essay on development by Samuel Huntington and regional overviews by Myron Weiner (on Asia, Africa, and the Middle East) and by Jorge Dominguez (on Latin America).

The second part comprises essays on political participation (emphasizing territorial groupings, ethnicity, and religion over the industrial working class and the peasantry) by Joan M. Nelson; agrarian politics (looking at peasants as rational actors) by Robert H. Bates; ethnonationalism (arguing that ethnicity patterns provide a means of understanding the development of nations) by Walker Conner; religion and East Asia (suggesting a return to culture and political behavior in contrast to a materialist and class view of politics) by Winston Davis; and social-psychological approaches to political development (questioning traditional emphasis on individual attitudes in contrast to corporate and collectivist traditions) by Ali Banuazizi.

Part 3 focuses on the once forgotten concept of state with essays by Peter Evans on foreign capital and its impact on the state in the third world; Eric Nordlinger on state autonomy; and Joel S. Migdal on the strong-weak state dichotomy. The very inclusion of these essays implies recognition that the state is a core concept in the study of politics and that it is time to move on beyond the belief that political system supplants the state, as once advocated by the Comparative Politics Committee of the Social Science Research Council (SSRC).

The final section contains an overview essay by Gabriel Almond, who asserts an ideological defense of past themes and mainstream political science, together with criticism of theorists who emphasize dependency, the state, and a class conception of politics.

This anthology of original essays is useful in several respects. First, it represents a long-awaited reassessment of ideas and literature that have long dominated the mainstream of political science. Second, it incorporates (largely in critical fashion) some of the dependency ideas that challenged the perspectives cast by Almond, James Coleman, and Lucian Pye two decades ago, ideas that were subsequently assimilated into mainstream political science. Third, it includes attention to the question of state. Thus the views of both older and newer scholars are linked in one volume. While there is reference to some of the views that have transcended the dependency school of thought, the volume is limited by its failure to deal seriously with some new thinking, for example, theories of mode of production and internationalization of capital that have influenced other disciplines but have yet to impinge on political science, probably because comparative politics has not fully incorporated political economy approaches and alternative radical and Marxist perspectives.

Resistance to some of the new views is evident in the initial and concluding essays, by Huntington and Almond respectively. Huntington is rather conciliatory, however, as he shows how liberal, dependency, and Marxist theories all focus on poverty and injustice. He stresses the relevance of various cultures in an explanation of developmental patterns and calls for closer links between comparative politics scholars and area specialists. Whereas Huntington acknowledges that the model of the developed Western society—wealthy, equitable, democratic, stable, autonomous—may not be useful to other societies, Almond is explicit about his Western preferences.

Almond carefully reviews and defends the contributions of two decades ago, including the influence of Talcott Parsons, the work of Coleman, Pye, and himself, and the SSRC series on political development. He rebuts the attack of dependency writers against mainstream comparative politics by erroneously characterizing all dependency writing as Marxist in its emphasis on class struggle and Leninist in its attention to imperialism; then he shows—by ignoring or not being aware of the plethora of criticism independent Marxists and Leninists directed against dependency theory over the past 15 years—that dependency theory was not very good Marxism-Leninism. Almond rests his critique on the dependency approach as "a propaganda fragment of an ideology, a polemic against mainstream development theory" (p. 454). Indeed the dependency approach served as an alternative to the old mainstream ideas, but it should not be forgotten that it also was an alternative to orthodox Marxist-Leninist theory as espoused by the traditional pro-Soviet Communist parties throughout Latin America and the third world. For all its failings, it stimulated new thinking and ideas and a healthy debate among third-world intellectuals and practitioners alike, and Almond ignores the substantial theoretical and empirical contributions that followed in the wake of the dependency tradition. Instead he concentrates on North American scholarship about dependency, decrying the ideological assumptions and the oversimplification of prominent writings such as Peter Evans's study of Brazil. He goes on to attack the "metaphysical overtones" of the "neostatist" movement led by Evans, Theda Skocpol, and others, arguing that his own pluralist and structural-functional movements had already demystified and operationalized the concept of state. Finally, he concludes that mainstream comparative politics is not in crisis.

Almond's forceful presentation serves as a staunch defense of the mainstream. While it may convince some to follow in this tradition, it certainly will provoke others to seek new alternatives.

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Creating Opportunities for Change: Approaches to Managing Development Programs. By Louise G. White (Boulder: Lynne Rienner, 1987. xvii, 283p. $32.00).

Listen to the People: Participant-Observer Evaluation in Development Projects. By Lawrence F. Salmen (New York: Oxford University Press, 1987. x, 149p. $24.95).

Both books aim to help policymakers and public administrators improve the contribution that development programs or projects can make to the economic, social, and even political well-being of third-world people. Both were written with the support of international donor agencies, the U.S. Agency for International Development and the World Bank, respectively. Their similarities end there. The first, in the tradition of public administration, addresses management problems "from above," while the other offers us a view "from below." It adapts the anthropological method of participant observation to inform managers how to meet better the needs and wants of the publics they are expected to serve.

The first will be of more interest within the discipline of political science because of its subject matter and the literature it draws on. Good use is made of the work of many political scientists, some fairly well known in the profession, such as Graham Allison, Robert Bates, Milton Esman, Elinor Ostrom, David Leonard, John Montgomery, and Aaron Wildavsky. I counted over 40 others whom I know, and there are probably more. Louise White brings in also the work of many other social scientists whom political scientists read—Argyris, Arrow, Buchanan, Downs, Hirschman, Lindblom, March, Selznick, and Simon, among others; her analysis is not only grounded in concerns of interest to political scientists, but she integrates them with concepts and conclusions from cognate disciplines. The book indeed provides a thorough review and critique of the current literatures that contribute to the multidisciplinary subject of development administration. It is a well-written statement of the state of the art, systematic enough to qualify as "science."

White makes a substantial contribution through her delineation and analysis of six approaches for improving development administration. The book offers something like a factor analysis of the literature, parsing it into six reasonably distinct and enlightening schools of thought about how to improve the management of development. At first I resisted her classification because I found the "vector labels" (my term, not hers) not conceptually clear (as usual with factor analyses) and because the categories had some lingering commonalities. But ultimately I was won over by the typology because the differences delineated are significant.

Each approach is discussed in its own chapter, including the relevant literature and also the criticisms and limitations associated with that approach. While White may have some favorites among the six, she presents each even-handedly and constructively, showing how the approach can be made most useful and complementary to the others. Each has some similarities or parallels with others, but enough substantial distinctions are detailed to make this taxonomy one to be reckoned with for years to come.

Space limitations do not permit a discussion of each of the approaches. The one that most intrigued me was "management by anarchy," inspired in part by Charles Lindblom's work on "muddling through" 25 years ago but advanced furthest by James March and associates in the last 10 years. I was struck by the possibilities this approach offers for linking with the work emerging from many disciplines on "chaos" (see James Gleick, *Chaos: Making a New Science* [1987]). "Wicked problems," for example, as identified by Rittel and Webber ("Dilemmas in a General Theory of Planning," *Policy Sciences* 4 [1973]:2) should be more amenable to analysis and action if understood in terms of the dynamics of "branching." The qualitative mathematics of "fractals" offer an alternative to the "linear" concepts and methodologies so dominant but inappropriate for most problems of management today. But each of the other approaches as mapped out by White also offers some new intellectual and experimental possibilities, so that public administration, relatively dormant theoretically for many years, could become more exciting as a field.

It is in its own way quite a breakthrough, for an anthropologist, Lawrence Salmen, to become so engaged in the task of improving bureaucratic approaches to working with the poor and for the World Bank to fund such a grass roots endeavor as *Listen to the People.* Political scientists who study micropolitics in the third world will find it of interest for its systematic exposition of the methodology of participant observation. Others may be interested in its documentation of the many ways "politics" intrude in the planning and implementation of development projects. Salmen lived in urban slum and resettlement areas in Ecuador and Bolivia to test and develop the methodology for World Bank use. He shows how donor agency ignorance of political factions, histories of past confrontation between government and local groups, patron-client systems in the barrio, electoral competition, local jealousies, and so on, impeded sensible project design and successful project implementation. Enough benefit was demonstrated from this investment in ethnographic data gathering (about .1% of total project cost) that the methodology was tested in several other countries and will now be used more widely by the World Bank, at least for urban development projects.

NORMAN UPHOFF

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Planning and Federalism: Australian and Canadian Experience. By Kenneth Wiltshire (St. Lucia: University of Queensland Press, 1986. x, 333p. $42.50).

This is not a book for beginners. As the author states at the outset, it requires "complete familiarity with the history and institutions of Australian, and to a lesser extent Canadian, federalism" (p. 4). It also requires some familiarity—and a lot of patience—with the manifold theories and models of public administration and planning. Thus it is a book for specialists; but this is not to say that it would not address and add to an important aspect of the study of federalism in general.

Wiltshire's central message is simple and clear: planning and federalism are essentially incompatible. The division of powers in a federation is "a fundamental hindrance to the attainment of public sector planning on a national basis" (p. 108). This cannot be otherwise because planning requires rationality and predictability, while federalism means fragmentation and uncertainty (p. 259). Since the division of powers cannot be eradicated, the incompatibility of planning and federalism can at best be "mitigated" through a number of reforms aiming at the reallocation of functions and responsibilities among the different levels of government and at the improvement of the "linkages" that in modern federal systems have been placed in between these divided jurisdictions: intergovernmental, and especially financial, relations.

The design of the book is equally clear and straightforward. Chapters 1–4 deal with the concepts of planning and federalism, with the planning experience in Australia (or lack thereof), and with a comparison of the federal systems in Australia and Canada. Here Wiltshire provides useful descriptions of the historical genesis of both federations, and of their major institutions. In chapter 5 he examines more specifically the division of powers in both countries, which eventually leads him to a lengthy theoretical discussion of functional reallocation. Chapters 6–8 then deal with intergovernmental agreements, the basic dilemmas of federal finance, and the specific problems of conditional funding. The "Conclusion" is really a misnomer, because this is where Wiltshire develops extensive thoughts about federal reforms that would improve planning capacities.

The entire study is not so much a comparative analysis as a well documented critique and conceptualization of planning in federal systems. Because of their constitutional similarities, Australia and Canada supply the empirical background. Nevertheless the author provides plenty of data based on government documentation, literary review and a substantive number of interviews. Here the specialist will find useful information on patterns of intergovernmentalism, fiscal transfers, and conditional funding in both countries, although the material is presented country by country rather than aggregated into a comparative analysis. What amounts to rather difficult analytical reading, interspersed with frequent conceptual excursions, is facilitated by helpful summaries at the end of sections and chapters.

This conceptualization is the most problematic aspect of the book. Delving into a rather dated discussion on "coordinate" federalism, "duality," and the "existence of a balance of power" at the outset (pp. 70–84), Wiltshire later has to present as a result of the interviews he conducted what in reality has been standard wisdom for quite a while: that due to "growing interdependence" federations are "no longer coordinate" but have become cooperative and intergovernmental, or what he awkwardly calls "organic" (pp. 127–28). Far from being coincidental, the choice of terminology helps to clarify the underlying rationale of the entire book. As "planning means coordination" and "coordination means hierarchy" (p. 31), federalism is incompatible with planning precisely because of its "lack of [hierarchical] coordination" (p. 84). This is where the student of federalism, among others, has to take exception. Only from a very narrow view of planning and public administration is it possible to argue that coordination means hierarchy. In the context of federalism and democracy, however, this is a contradiction in terms.

If there is a lack of coordination in the Australian and Canadian federations, it is more likely a lack of nonhierarchical and cooperative coordination caused by the hierarchical planning bias of government bureaucrats trained in the centralized Westminster tradition. Not surprisingly, Wiltshire's competent analysis of the intergovernmental linkages in modern federal systems leads him to the self-evident observation that the "classical" and "dual" theories of federalism are dated. Perhaps it is fair to say that in a modern world of fragmentation and interdependence the "classical" theory of linear and hierarchical planning is just as dated.

THOMAS O. HUEGLIN

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INTERNATIONAL RELATIONS

Terrorism, U.S. Strategy, and Reagan Policies. By Marc A. Celmer (New York: Greenwood, 1987. x, 132p. $29.95).

The now vast literature on political terrorism abounds with proposals their authors think governments should adopt in order to prevent terrorist events from taking place and for defeating the aims of their perpetrators. It also includes many commentaries on the various measures governments have already used to achieve these goals; usually these are accompanied by assessments of their effectiveness. Compared to these efforts, the body of writing whose focus is not on antiterrorist policy but on the mechanical or bureaucratic aspect of the problem is very limited. The book under consideration here seeks to fill this gap by reviewing the various organizational, civilian, and military arrangements and rearrangements the U.S. government has experienced as recent presidents have sought to combat terrorism.

Given its contents, an appropriate subtitle for this volume might be *How an Elephant Fights a Mouse.* As related by the author, the technique goes something like this: After the elephant is bitten, it emits a loud trumpeting sound. This is followed by a long pause as the gigantic animal contemplates further reaction. The elephant glances toward other members of the herd who were also bitten. After a time, the elephant's brain sends a series of messages, known as EOs and NSDs, to the rest of its body. Tusks are raised and lowered, the elephant's legs stomp the mud until it is satisfied that the mouse has been sufficiently impressed to cease its attack. There is another pause until the mouse bites again. This time the trumpeting becomes louder, but the behavior that follows looks remarkably like the earlier reaction. The mouse, though injured, lives on. Apparently the elephant has even grown fond of the little creature. Why else would it lately have given the mouse a small portion of cheese?

Moving from the realm of parable to a more prosaic level, it seems clear that the organizational response to the problem posed by political terrorism has been event-driven. Beginning with the Nixon administration's reaction to the Munich and Lod Airport massacres in 1972, a succession of U.S. presidents, with some congressional participation, have created policy-making and bureaucratic frameworks within which to mount antiterrorist campaigns. Each new administration has modified the arrangements created by its predecessor. Also, there has been a rapid rate of turnover in the personnel given the task of organizing the response. For the most part these changes have been stimulated by presidential dissatisfaction, itself brought on by renewed terrorist attacks on U.S. citizens and facilities.

This is the story told in the book. The volume is marred by its author's unrelenting use of bureaucratic jargon, replete with the usual alphabet soup of agency names. A more thorough editorial effort would have helped.

LEONARD WEINBERG

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The Vatican, Islam, and the Middle East. By Kail C. Ellis (Syracuse, NY: Syracuse University Press, 1987. 364p. $39.95).

This volume consists of essays presented at a conference on the subject of the Vatican, Islam, and the Middle East at Villanova University. Conference collections can present certain difficulties. The generality of the title allows for the coverage of a wide variety of themes yet conveys a certain lack of focus. Father Kail C. Ellis's introduction categorizes and provides a conceptual framework for the conference papers. He makes it clear there are certain things this book will not do: (1) This collection will not focus on the Holy See only as a classical sovereign state model. Although the Vatican may be viewed as a transnational actor in the traditional political science sense (see J. Bryan Hehir's article, 109–24), it is principally by virtue of its moral authority that it plays a significant role in the Middle East today. (2) The reader should not expect new insights into the Vatican's foreign policy-making processes. They remain a mystery. Ellis's broad claim that this volume offers the reader a multifaceted, coherent approach to the issues of Christian-Muslim relations and the Vatican's approach to the Middle East masks a disparity of topics. Stylistically, the chapters range from comparative theosophy, traditional political analysis, and historical, conceptual, and philosophical analysis to case studies of Christian-Muslim relations. Finally, the quality of the chapters is uneven. All tend to focus more on description and categorization than analysis or significance. The most original is James A. Bill and John Alden William's comparison of similarities in the doctrinal, structural, and sociopolitical characteristics of Shi'i Islam and Roman Catholicism. Especially valuable is the authors' comparison of the Catholic and Shi'i models of politics and the state. Bill and Williams observe certain similarities between populist Islam as seen among Shi'i communities and liberation theology as practiced by some Roman Catholic clergy, especially in Latin America. Hehir examines the role of the Vatican as a transnational actor operating at three separate levels of analysis: center-periphery, periphery-center, and periphery-periphery. This is a convenient taxonomy for analyzing the role of the Vatican as a political actor, not a nation yet having its own interests in the Middle East. Some articles are weak in conceptual clarity or depth. Joseph L. Ryan's article is a review of contacts between the Holy See and Jordan from 1953 to the present. He defers treatment of the wider political context—as well as analysis of the significance of each contact—to the political and diplomatic historian. Yoakim Moubarac seeks to apply the concept of minority to Lebanon but his differentiation among minorities, unionists, and prototype communities (those who created Lebanon) lacks in precision as well as raison d'etre. Moubarac's thesis that the Maronites' political plan sought to create an egalitarian and modern Islamic-Christian society to serve as a prototype for the nation-states of the Arab East may be influenced by his own background as a spokesman for the Maronite church. Moubarac's article is a philosophical and conceptual treatise lacking in internal analytical coherence. In the chapter on Pakistan, Joseph Cardinal combines the style of an historical essay with inspirational suggestions to Christians for survival in an overwhelmingly Muslim state.

The intent of the conference (to examine the new Catholic posture toward Islam) makes this collection useful to the general reader and student, if not to the researcher. The Catholic church's emphasis on Christian-Muslim cooperation and dialogue derives from the statement, *Nostra Aetate,* issued by the Second Vatican Council (1965). This declaration stressed the common beliefs of Muslims and Christians and sought to discredit a stereotyped view of Islam as a crusading religion of power and ignorance. It is as a religiopolitical institution that the Catholic church enters into dialogue with other religions, as a "witness of faith in the world" (p. 2) and as a transnational actor.

The relationship of Catholicism and Islam has attracted little attention in the literature. One exception is George Irani's recent book, *The Papacy and the Middle East* (1986). Yet no other area of the world has been the focal point of such prolonged Vatican interest. The Vatican maintains diplomatic relations with several Arab and Islamic states and exerts moral authority on issues that vitally affect Christians and Muslims.

Four aspects of the progress of Catholic-Islamic cooperation are examined in this volume: (1) Islam and Western Christianity, (2) Vatican diplomacy and the Middle East, (3) the Vatican and Muslim-Christian relations in the Middle East, and (4) resurgent Islam and Christian-Muslim relations. Although the quality and originality of the chapters vary considerably, those by George Emile Irani and Fred J. Khouri provide useful background and information on two of the most intractable problems of the volatile Middle East: the Israeli-Palestinian conflict and Jerusalem. According to Irani, the Holy See has been sympathetic to the plight of the Palestinians from the beginning of the Israeli-Palestine dispute. This position was motivated by the Vatican's concern for the fate of Catholics in Palestine and the humanitarian needs of the Palestinian refugees. Khouri traces the Vatican's concern for Jerusalem from the British mandate period to the post-1967 phase. During this time, the intent of Vatican policy shifted from internationalization *(corpus separatum)* to one of guarantees for freedom of worship, respect for the Holy Places, and preservation of the Christian presence in the Holy Land. Khouri warns against ignoring the status of Jerusalem, since no lasting solution to the Middle East conflict can be reached without an acceptable agreement on East Jerusalem.

The last two sections of this book consist of case studies. Individual authors discuss the status of small, marginal Christian communities in Pakistan and India—the first overwhelmed in a predominantly Muslim state, the second in a Hindu-dominated but religiously pluralistic state. Others examine the reverse of that situation: the status of the quasi-insurgent minority Muslim community in the Catholic Philippines. Thus, geographically as well as analytically, the topics explored by the various conferees ranged widely.

Perhaps the greatest contribution of this collection is its emphasis on the role of the Vatican in promoting mutual understanding between Muslims and Christians. In recent times, the Iranian revolution and the Shi'ite movement in Lebanon have contributed to distorted and stereotypical images of Islam. The essays in this volume serve to present Islam in its proper perspective as a religion of piety, patience, charity, and human concern that claims over 800 million adherents. Ellis also points out that Islam is not just a religion confined to distant lands but a U.S. religion. By the year 2015, it may become the second largest religion in the United States (p. 21). The insights and dynamics presented in this book underscore the importance of establishing a sound basis for Christian-Muslim relations. As the editor states, such mutual understanding can be achieved through the common theological heritage of one God and the common Abrahamic tradition of Judaism, Christianity, and Islam.

CAROL J. RIPHENBURG

Mankato State University

War and Intervention in Lebanon: The Israeli-Syrian Deterrence Dialogue. By Yair Evron (Baltimore: Johns Hopkins University Press, 1987. x, 246p. $39.50).

This book adds, in a captivating way, one more piece to the puzzle of the Lebanon war of 1982. It reinterprets the politics of Israel and Syria in Lebanon between 1975 and the present through the prism of mutual deterrence and is a scholarly testimonial to the often-repeated notion that Israeli-Syrian relations, though hostile, are based on a pragmatic, almost mathematical, set of operational guidelines.

The material covered in yet another volume on the Lebanon issue is not new. However, Evron makes an original contribution to the role of regional politics in Lebanon.

The perspective used by Evron strips the complex Israeli-Syrian relationship to its core. This is both an advantage and a liability. On the one hand, one can clearly see the evolution of a mutual deterrence game played by two local hegemons on the Lebanese scene, replete with definitions of interests, tests of wills, escalations and deescalations, although, as the author reminds us, the success of deterrence "is usually much more difficult to identify and prove than its failure" (p. 176). Yet one cannot but realize that if this "deterrence regime" has succeeded, it is in part because the presidency of Syria's Assad has endured the test of time. In this sense, the deterrence equation is one tying Israel and Assad rather than Israel and Syria.

On the other hand, one cannot but wonder whether it is possible to reduce the multifaceted protracted social conflict raging in Lebanon since 1975 to what is essentially an Israeli-Syrian dyad. To what extent, we must ask, is war and peace between Israel and Syria purely the factor of a deterrence equation rather than the outcome of different competing domestic, regional, and international images of security and national identity? This is particularly true when both powers are entangled in a country with no authority of its own. This was made clear during the clashes over Zahle (which Evron euphemistically refers to as the "Zahle encounter"), which led to Israel's downing of Syrian helicopters, and Syria's introduction of surface-to-air missile batteries into Lebanon's Bekaa valley. Then, the records show, Lebanon's Bashir Gemayel was intent on provoking a clash between his Lebanese forces and Syrian troops, thereby forcing Israel to come to his support. In this case it became incumbent upon Israel to respond militarily.

In 1978 a different but equally important chain of events had led to the previous major Israeli incursion in Lebanon. That incursion was primarily in response to a Palestinian guerrilla raid, which had resulted in 35 Israeli deaths, most of them civilian. Ironically, the chain of events that led to the "Litani Operation" was triggered *because* of the deterrence regime. In south Lebanon the PLO had become a source of worry to Israel because "Syrian forces were not allowed into the south by the Israeli deterrence threshold; the Lebanese government was unable to extend its authority into the south, and the local Shi'i population was incapable of resisting PLO movements there" (p. 71).

On Israeli and Syrian deterrence, Evron states that "it is difficult to prove whether the threat to escalate the conflict has been the basis of Israel's successful deterrence of a limited Syrian initiative" (pp. 197–98). It is certainly no coincidence, however, that Lebanon's border with Israel became an active guerrilla zone, and that the incidence of cross-border clashes rose sharply at the same time as the development of the deterrence regime. One can therefore suggest that as the cost of direct conflict between Syria and Israel outweighed its benefits, the nature of the conflict shifted to a different level. In Lebanon, Syria and Israel could confront and deter one another through their allies while minimizing the risks of outright war. Incidentally, that regime is still operational.

It is a little unfortunate that the editors were rather inconsistent with the nomenclature. Rather than being transliterated from Arabic into English, Arab names are often written as they sound in Hebrew, and even then, printed inconsistently. Thus, Camille Chamoun also appears as *Kamil,* Haddad is alternatively written *Hadad,* and Lieutenant al-Khatib is referred to as *Hatib.*

Despite this minor objection, Yair Evron's *War and Intervention in Lebanon* is a welcome addition to the conflict management and deterrence literature.

EDWARD E. AZAR

University of Maryland, College Park

Normative Politics and the Community of Nations. By Haskell Fain (Philadelphia: Temple University Press, 1987. xii, 244p. $32.95).

The aim of this book is to sketch a new theory of international ethics, one that differs from existing theories in several ways. Unlike political realism, Fain's theory does not arbitrarily elevate national claims above those of the international community. Nor does it challenge the moral legitimacy of national sovereignty in contrast to the view that boundaries count for little in determining international moral obligations. Fain is willing, as realists and globalists often are not, to talk about the rights and duties of states. But while most theories of international obligation derive such rights and duties either from consent ("contractarianism") or from the constitutive practices that define and establish the community of nations ("legal conceptualism"), Fain rejects these derivations as spurious. The true source of international obligation, he argues, is the existence of certain "morally necessary tasks" that can be performed only at the international level. The nations of the world comprise a moral community because—and only because—their cooperation is required in order to realize these vitally important goals.

Fain devotes a good deal of effort to undermining contractarianism, which he thinks is defective because it fails to recognize that the goals of a community are prior to the rules and rights they legitimize. The right to property, for example, is something that must itself be morally justified in relation to the good of the community. Political communities are not founded upon the inviolability of contract but upon the duty to perform morally important tasks. Indeed, the very idea of a morally binding social contract presupposes the idea of morally imperative purposes or tasks.

Fain's attraction to a goal-oriented, or teleological, approach to ethics explains his skepticism regarding legal conceptualism as well as his rejection of contractarianism. Legal conceptualism is the view that rights and duties are derived from antecedent rules and that there can be no political community apart from the legal rules that constitute it. Fain at times seems to favor this view, since he argues that contracts "presuppose that one is not in a state of nature with regard to those with whom one wishes to contract" (p. 89). But he does not take this to mean that contracts presuppose rules and procedures establishing the institution of contract. Fain thinks such reasoning is mistaken. Morally speaking, it is not rules and procedures that undergird political communities and their institutions, but purposes. Morally justified powers are derived not from rules, which are instrumental and secondary, but from the primary moral purposes that provide the root justification of all legal and political arrangements.

Fain's own view, then, is that political communities are constituted by their "proper tasks" (p. 199). One example will suffice to suggest the sort of difficulty with which this theory must deal. "A chess club, which is governed by a steering committee," Fain writes, "is a political community that is constituted by 'the task' of seeing to it that its members have the opportunity to play chess, and if the steering committee uses the money of its members for any other purposes, it is acting *ultra vires*" (p. 199). But to act *ultra vires* is to act in a manner beyond the powers authorized by law. The legal conceptualist will therefore object that we can't determine whether a steering committee is acting *ultra vires* until we know the charter or constitution of the association. In deciding whether a government is acting within its authority one consults these rules, not what some undefined body of opinion deems to be its proper tasks. Furthermore, as everyone knows, associations often change their goals.

Fain's central thesis is that if there are morally essential tasks that can be pursued only through international cooperation, there exists a global political community whose reality and raison d'etre derive from the moral obligation to perform these tasks. What are these tasks? According to Fain, the international community is constituted by such tasks as eliminating war, achieving a "more satisfactory" distribution of the earth's resources, and preserving these resources for our descendants (pp. 184, 204-5). The general problem, of course, is to determine which tasks are moral tasks, tasks that are constitutive of the community. If the moral authority of law is accounted for by its "communal desirability," how does one determine what is desirable for the community? Fain acknowledges the difficulty: "I do not have a general theory that could explain, in a rigorous way, exactly which of the possible tasks of the state could be moral tasks. . . . I do not believe there can be such a theory, for it seems to be history, rather than theory, that determines which tasks are morally important for a given political community" (p. 201).

As a moralist, Fain repeats some common complaints about the present international order. I am not convinced that he has succeeded, as a theorist, in providing a solid foundation for these complaints.

TERRY NARDIN

University of Wisconsin, Milwaukee

Nuclear Deterrence, Morality, and Realism. By John Finnis, Joseph M. Boyle, Jr., and Germain Grisez (New York: Oxford University Press, 1987. ix, 429p. $39.95.)

Is it proper for the governments of the United States, Great Britain, and France to threaten millions of Soviet citizens with nuclear incineration in order to dissuade the Soviet government from aggression that might otherwise occur? These three Catholic writers argue thoughtfully, relentlessly, and persuasively that nuclear deterrence is not right, that it is flatly immoral because it egregiously offends the "common morality" of Western civilization.

British lawyer Finnis, U.S. moral philosopher Boyle, and U.S. theologian Grisez base their condemnation on nuclear deterrence's clear violation of this civilization's absolute prohibition of the deliberate killing of innocents. As the authors show, nuclear deterrence involves the continuously expressed threat to wipe out the cities of the USSR, a conditional intention to kill millions of Soviet civilians if their government "makes it necessary." And this murderous intention is backed up with appropriations, deployment of the vast arsenal of nuclear tools, elaborate planning and preparations, indoctrination and training of thousands of military personnel who share the intention, and some predelegation of authority to enable subordinates to unleash the devastating nuclear strikes; so nuclear deterrence cannot be excused as the chief of state's personal bluff, a threat that might be without sin because he or she might not really mean it and might not actually execute it "on the day."

The authors reject consequentialist arguments that defend deterrence as a necessary, so-far effective, and therefore morally justifiable means for defending respectable Western values in the current emergency situation brought on by the menacing behavior of the repugnant Soviet regime. To the consequentialist suggestion that nuclear deterrence is preferable to prompt nuclear disarmament that might be followed by Soviet destruction or domination of the West, the authors elaborately explain how and why "the consequences of deterring and disarming are unpredictable, the probabilities of their occurrence are unknowable, and so the net utilities are incalculable" (p. 243). Consequentialism is revealed as an incoherent methodology. The notes following these middle chapters are a gold mine of serious grappling with the best points that consequentialists have been able to make.

The authors do not shrink from the possibility that renouncing nuclear deterrence might result in Soviet domination. In fact, they probably exaggerate the awful things Moscow would be likely and able to do to the West if one of these days the Kremlin's bosses awoke to find the West's nuclear forces had been abandoned, especially if the West had concurrently increased its conventional military forces, developed various forms of alternative defense and civilian resistance capabilities, and sharpened its economic and diplomatic instruments. But even if renunciation and abolition meant Soviet domination, the authors are convinced that nuclear deterrence must be condemned and abandoned as a morally prohibited means, however good the ends and unappealing the possible consequences. Civilized people are expected to suffer evil rather than to inflict or even to threaten or intend it.

Much of their discussion of the philosophical and theological origins of their central principle absolutely forbidding the killing of innocents could support a more thoroughgoing pacifism rather than their continued acceptance of just war doctrines that sound dubious alongside unabashed assertions about the sanctity of human life, the uncertain purposes of even "good" wars, and Christian faith in God's unknowable plans (part 4). But if these two chapters provoke question marks in the margin, the essay as a whole is an excellent, sophisticated contribution, perhaps the best that has been set out among the many recent books on this difficult but crucial subject.

JAMES A. STEGENGA

Purdue University

Refugees in International Politics. By Leon Gordenker (New York: Columbia University Press, 1987. 227p. $30.00).

Leon Gordenker laments the steady increase in forced migration but finds intrinsic value in new forms of international cooperation to address refugee problems. His study briefly charts the growth of intergovernmental and private refugee institutions in the twentieth century before classifying the various causes of and responses to refugee emergencies. Initially major international wars were the primary cause, but since 1950 civil war, human rights violations, technological advances, and the consolidation of national power in former colonies have generated the major outflows.

Extraordinarily Machiavellian regimes have deliberately expelled selected groups of their citizens. Countries receiving forced migrants may pursue their own political ends by settling refugees on the border in bases calculated to destabilize an unfriendly neighbor. Permanent intergovernmental refugee institutions have replaced the first temporary agencies, and private humanitarian groups have become increasingly skilled. Despite improved international collaboration, Gordenker recognizes that existing institutions are so inadequate that further cooperation is urgently needed.

Gordenker examines overlapping concepts such as *refugee* and *migrant*, which have acquired confusing, politically dictated meanings with legal consequences. Not all migrants forced to leave their homelands are recognized as internationally protected refugees. The United States, for example, distinguishes economic refugees and individuals fleeing civil war from those entitled to asylum as a result of political persecution. Mass exoduses resulting from natural disasters create a distinct category with equally pressing needs for emergency relief. United Nations and nongovernmental agencies initially granted a narrow mandate have progressively expanded their programs to assist otherwise unprotected groups when distinct needs arose.

The title reference to "international politics" does not accurately reflect the work's primary focus on transnational organization. Gordenker favors the humanitarian spirit motivating cooperative relief efforts and does not fully explore the national political interests and ideological differences affecting refugee situations. Without adequate explanation the text notes that the Soviet bloc's nonparticipation in international refugee organizations and fails to mention the unique tradition of political asylum among Latin American governments. The useful description of how political factors led African states to enlarge the legally recognized class of refugees is appropriately contrasted with the more narrow concept advanced to serve Western interests.

Gordenker correctly shows that political considerations have resulted in minimal legal protection for refugees. Nevertheless, the standards that have been codified deserve more systematic and less summary treatment. Richard Lillich's *The Human Rights of Aliens in Contemporary International Law* is a particularly valuable reference that is not cited.

The work presents numerous specific examples of refugee problems involving a variety of governments from different regions, which provide valuable illustration for the author's general classifications and observations. The study could have better shown the complex interactions among intergovernmental, national, and private relief agencies with one or more in-depth studies. Repeated brief references to the UN high commissioner for refugees create a fragmented, slow-to-develop picture of that all-important agency. Generalizations revealing the author's commonsense understanding of international affairs occasionally read like statements of the obvious (e.g., "Resettlement inevitably involves costs"). Concluding chapters too often unnecessarily repeat illustrations and analyses from earlier portions of the work. Humanitarians concerned with suffering refugees are occasionally faulted for addressing symptoms without remedying the underlying cause. Aside from a brief discussion of "early warning" mechanisms, Gordenker finds little prospect that refugee problems can be prevented.

Gordenker relies primarily on official documents and regrettably makes few direct references to his interviews with refugees, public and private relief officials, and four of the UN high commissioners for refugees. His normative, general overview offers minimal empirical analysis of data on refugee populations or relief programs. Recognizing the need for original research, the author does offer specific recommendations for future studies employing quantitative methods. The book concludes by suggesting that the transnational activity described constitutes an emerging international regime, suggesting an approach that might have been employed to give a somewhat greater analytical focus to his work. Gordenker's broad overview and general description provides a useful introduction to a subject of growing importance as well as helpful recommendations for future research.

HOWARD TOLLEY, JR.

University of Cincinnati

New Directions in the Study of Foreign Policy. Edited by Charles F. Hermann, Charles W. Kegley, Jr., and James N. Rosenau (Boston: Allen & Unwin, 1987. xiii, 538p. $60.00, cloth; $19.95, paper).

This study contains a critical evaluation of the field of comparative foreign policy, which Rosenau originated in 1966. It is the outcome of a conference at Columbus, Ohio in May 1985, sponsored by the three editors' institutions: the Mershon Center and the universities of South Carolina and Southern California. Twenty-two papers have been revised by contributing scholars and carefully edited to present innovative proposals for research. There are no contributions from outside the United States.

A strength of this study is its criticism of the field. The major problem is that comparative foreign policy lacks a cohesive theory. In 20 years it has produced many hypotheses about isolated phenomena but no comprehensive body of thought or overall explanation. Nor is there any clear definition of content. It intersects both international relations and domestic decision-making behavior. It is thus a bridge between complex disciplines.

The editors are optimistic that a theory may emerge, and they give encouragement here to some proposals. Bruce Moon proposes basing comparative foreign policy in political economy, that is, understanding the economic determinants of behavior. Rosenau's reply to the challenge of general theory retreats to a single-country theory, which he illustrates with the USSR, ingeniously resolving the unique and the general by calling for a dialogue of the cultural historians and the international theorists.

The most comprehensive response is advanced by Maria Papadakis and Harvey Starr. Their model incorporates the three basic elements of process, output, and behavior: First the process of decision making is done by the actor (the state, decision makers, and structures), operating within the opportunity provided by the environment and constrained by it and by the actor's perception of the environment. Secondly, the actor's choice determines the output (decision). Thirdly, the actor's choice further determines outcomes (actual behavior). These then affect the environment, divided into six major levels of rising complexity—four domestic (individual, role, governmental, and societal) and two international (international relations and international system).

The other essays deal with one part of the field. In the area of process, Charles Powell, James Dyson, and Helen Purkitt urge the necessity of learning how policymakers understand a problem. Dwain Mefford proposes the use of artificial intelligence and shows that analogical thinking is pervasive and basic to decision making, as in the 1968 Soviet intervention in Czechoslovakia. In social organization Kegley proposes the study of regimes, Stephen Walker examines the use of role theory, and Paul Anderson studies social process of policy-making in three U.S. crises. Charles and Margaret Hermann join Joe Hagan to identify three types of decision-making bodies.

In the area of outcomes, William Dixon demonstrates a lag sequential approach to US-USSR behavior between 1966 and 1978. Gregory Raymond proposes evaluating consequences defined by goals. David Bobrow and Steve Chan urge study of anomalous "successes," such as three Asian states. Unfortunately, success is weakly defined by selectively quoting some pessimistic expectations of the 1950s. Benjamin Most and Randolph Siverson show that the intuitive belief that arms races and alliances are interchangeable does not apply between 1870 and 1914. Focusing on the interaction of states, Neil Richardson urges study of dyadic interaction and shows how it works in promoting conflict to divert from domestic difficulties. Russell Leng shows how behavior in militarized disputes corresponds with the structure.

Three approaches to international influences on the environment are advanced. Papadakis and Starr use behavior of small states. Eugene Wittkopf and Mark DeHaven test the influence of Soviet behavior on U.S. presidential popularity. Margaret Karus and Karen Mingst show that all states are influenced by international organizations.

Among the domestic influences of the environment, we have new ideas from Hagan on how to study the effect of opposition, from John Vasquez on the interplay of accommodation and power politics in the outbreak of war, and from Martin Sampson on the importance of cultural influences illustrated by France and Japan. I am struck that so little is known of the actual processes of policy-making in these two countries. Although our knowledge of facts of international relations has grown greatly, we still lack some of the most critical data. Beyond that we need confidence that data is not arbitrarily classified and that the interrelationships of such a huge and complex field are correctly understood.

JAMES W. GOULD

Scripps College

Ideology and U.S. Foreign Policy. By Michael H. Hunt (New Haven: Yale University Press, 1987, xiv, 237p. $22.50).

Michael Hunt, a distinguished specialist in the history of U.S. relations with China, ventures beyond the bounds of his earlier writing to study how certain traditional ideas and prejudices have shaped the United States' behavior abroad. It is a work of history and theory together: a historical tracing of the recurrence of three characteristic U.S. habits of thought and also a theoretical discourse on the degree to which beliefs that are seldom consciously examined have shaped our foreign relations. The book begins with theory in an introductory chapter entitled "Coming to Terms with Ideology," then turns to history. Three themes of U.S. thinking—visions of national greatness, notions of racial superiority, and distrust of revolutions—are traced in three chapters through the seventeenth, eighteenth, and nineteenth centuries. A fifth chapter pursues these themes through the history of the twentieth century, then a final chapter titled "The Contemporary Dilemma" turns again to the theoretical problem of how U.S. citizens can be made aware of, and so be freed from, a "foreign policy ideology" that too often replaces thought and results in irrational behavior.

As readers of Hunt's earlier works would expect, the historical writing is superb. His prose is lucid and graceful, and he knows how to carry a narrative and an argument forward together. He warns his reader that he is experimenting, moving for the first time beyond the strict, source-tied writing of specific history to the exhilaration of summarizing decades in a page. Still, he is too well trained and experienced to lose his caution, and his generalizations are constructed carefully and substantiated as well as the broad scope of his inquiry will permit. Surely this volume will find a wide and appreciative audience among students of diplomatic history.

As a work of theory it will have its critics as well as its admirers. It will be applauded as an ambitious and resourceful attempt to expose the impact of popular prejudices and cherished assumptions on foreign policy thinking, but it will be faulted on at least two basic points.

First, do the three habits of thought Hunt identifies and traces really amount to U.S. ideology of foreign affairs? The reader senses that the whole of U.S. ideology is bound to be larger than the sum of the parts presented here. If so, what themes characteristic of U.S. thought have been omitted? Hunt, a long-time student of U.S.-Chinese relations, not surprisingly centers on U.S. citizens' sense of racial superiority. But it seems a student of U.S.-European relations would be inclined to feature their distrust of monarchies ("crowned ruffians," Tom Paine called them) as well as moral skepticism of balance-of-power politics, and a student of U.S.-Latin American relations would seem likely to center on hostility to the Church of Rome and condescension to what Jefferson called "those priest-ridden peoples." Rather than scanning for themes in the vast record of three centuries of experience, mightn't it have been a safer method to track certain persistent metaphors in U.S. discourse on foreign affairs? There is, for example, the metaphor of the covenant, ably explored in Robert N. Bellah's *Broken Covenant*. The metaphor of the United States as Jerusalem, as opposed to Old World Babylon, is studied in Ernest Lee Tuveson's *Redeemer Nation*. The metaphor of the U.S. experiment is discussed in Daniel Boorstin's *Genius of American Politics*.

Second, is *ideology* too broad and inexact a term for this collection of traditional assumptions? Hunt's definition is very loose: "an interrelated set of convictions or assumptions that reduces the complexities of a particular slice of reality to easily comprehensible terms and suggests appropriate ways of dealing with that reality." This definition is posited in the book's opening paragraph, labeled "commonsensical," and never tested or defended. I recommend William H. McNeill's notion of "public myth" ("The Care and Repair of Public Myth," *Foreign Affairs* 61 [1982]: 1-13) as a kindred but more focused concept that avoids the debate over the meaning of ideology and clarifies the psychological importance of these beliefs to the popular mind. McNeill's words on myth making and myth breaking speak directly to the problems Hunt raises in his concluding chapter.

Hunt modestly says that his aim is only to make a start at a neglected problem, that this is a first cut and an invitation to others to try their hand. It is a promising beginning. He has gathered a large and varied array of telling items and linked them in an insightful and instructive historical essay. His successors in this inquiry will want to refine his instrument of analysis, however. McNeill's essay is probably the place to start.

STEPHEN D. WRAGE

Georgetown University

International Aviation and the Politics of Regime Change. By Christer Jönsson (New York: St. Martin's, 1987. vi, 185p. $35.00).

This book is misleadingly titled; the emphasis should be on the politics of regime change, since the author's intent is to evaluate various explanations of regime establishment, change, and maintenance. International aviation is the issue area on which he focuses. Along with economic, structural, and situational models that approximate those previously used by Keohane and Nye in the evaluation of ocean and monetary regimes (*Power and Interdependence: World Politics in Transition* [1977], he introduces a model similar to a complex interdependence model, which he calls a process model. Like Keohane and Nye, he moves from the most parsimonious models to the more complex ones in his investigation.

The three phases that Jönsson studies are the establishment of the sovereignty-oriented air regime in 1919, the granting of some basic international freedoms in international air transport with the Chicago-Bermuda agreements at the end of World War II, and the unsuccessful challenge by the United States in the late 1970s to make international aviation considerably more open. Although the United States was successful in having pricing rules relaxed, Jönsson asserts that the United States' restricted success marked a change *in* the regime rather than a change *of* the regime. Consequently, he describes the U.S. challenge as a failure.

Jönsson's overall conclusion regarding the use of the four models is that because they each ask somewhat different questions and provide answers that at best represent only partial explanations, the models can complement each other. However, by generally showing the structural and economic models to be inconsistent and the situational one to be an awkward blend of complexity with a simplifying assumption of rational actors, he strongly encourages the increased investigation of regimes using the process model. With his process model, he sees regime dynamics as "outcomes of multilevel bargaining processes where the existence or absence of transitional networks is of special significance" (p. 153). It goes beyond both a state-centered and domestic politics approach in order to investigate the critical organization links for information flow in a bargaining process that seeks a formula or compromise. Bargaining theory and interorganizational theory intertwine in a complex way to explain how significant agreements emerge in regime decision making. By meshing bargaining and organizational theory in more case studies, Jönsson suggests that political scientists will be better able to generalize about the decision-making process in regime dynamics.

In response to Jönsson's suggestion that the four models be used to complement each other, the following question arises: Do partial explanations to different theoretical questions truly complement each other, compete with each other, present groundwork for synthesis, or simply illustrate the barrenness of theory in regimes?

Like Keohane and Nye, Jönsson tests models against a regime, but unlike them, he is only concerned with one regime. Although he is certainly not the only regimes analyst to focus on one issue area, his concepts of regime establishment, change, and maintenance would have been better developed with comparison across two or more issue areas—for example, with similar instances in Keohane and Nye's case studies.

Jönsson has written very clearly, with excellent transitions between his many models and eras. He has contributed to the regime literature with his description of the evolution of the air regime, and his work fits neatly in the international regime literature that has been so abundant in the last decade. Perhaps its very neatness is the biggest problem, though. He apparently assumes he can rush through his analyses of structural and economic models because they have already been generally attacked in the regime literature. His use of the situational model struck me as similarly half-hearted, although better developed. Overall, I think he could have made a much more significant contribution by concentrating only on the process model and emphasizing the theoretical ramifications of such a shift to complexity for explaining regime dynamics.

STEVEN E. ELSE

United States Air Force Academy

War in International Society. By Evan Luard (New Haven: Yale University Press, 1987. 468p. $32.50).

The Lessons of Recent Wars in the Third World, Vol. 1. Edited by Robert E. Harkavy and Stephanie G. Neuman (Lexington, MA: Lexington Books, 1985. ix, 304p. $35.00).

In a field dominated by quantitative analysis, it is unusual to find a large book from a university press eschewing quantitative analysis for old-fashioned intuition. It is even more unusual to find a book exemplifying the very practices that proponents of the quantitative method have criticized—definitions that are not precise, criteria that are not rigid, and generalizations that are too vague to be either verified or refuted. Because it offers so many examples of what the quantitative school criticizes, Evan Luard's *War in International Society* is unlikely to make inroads on its dominance.

Although providing some trenchant criticism of other definitions of war, Luard then offers one remarkable for its elasticity—an "encounter" involving "substantial" fighting over a "significant" period (p. 7). By including such diverse events as the Syrian leveling of several square blocks of their own city of Hama in 1982 and a 1981 border skirmish between Ecuador and Peru that killed nine, Luard has produced a very long list. The Correlates of War project identified 224 wars (interstate, extrasystemic, and intrastate) from 1816 to 1980; by contrast, that portion of Luard's list for the same period (and his overall list is much longer) covers 388.

Working from this list, Luard makes observations of frequently surprising vagueness. We are told that governments have not "normally" desired war for its own sake (p. 181) and that "in many cases" public opinion has proved more warlike than governments themselves have been (p. 224). The reader may grant that a false precision is misleading. Even so, the author should acknowledge that when unambiguous quantitative measures are available they help the reader assess the author's conclusions. Luard's assertion that "we live today not in a more peaceful international society but one that is particularly warlike" (p. 395) appears to contradict conclusions of other scholars (for example, the chapter by Starr and Most in the other book here under review), yet because he does not lay out the quantitative basis for his assertion, we cannot be sure. Only occasionally does Luard offer even percentages and then only as part of a running account, never in tables.

By choosing to begin in 1400, Luard has produced a list of staggering size. The relevance of much of this material to contemporary concerns will strike many readers as minimal. A reader totally unaware of history may find it useful to learn that states once went to war for dynastic concerns but do so no longer; readers who have grasped this point may question the value of learning the details of the particular dynastic wars.

His large number of cases leads Luard into a trap. Understandably, he divides the wars into periods. Less understandably, he gives each of these periods names—thus 1789 to 1917 becomes the "Age of Nationalism" while the period from 1917 to the present becomes the "Age of Ideology." He then allows these labels to influence his analysis. Wars fought since 1917 are perforce motivated by ideology. Thus, according to Luard, when Germany and Italy achieved unification through war, they fought wars of nationalism; when Vietnam and Korea sought unity through wars, they fought wars of ideology. In 1848 Hungarians fought a war of national liberation against the Austrians; in 1956 they fought an ideological war against the Soviet Union. Even the squabbling among rival warlords in Lebanon in 1958 is interpreted as a war between Communists and anti-Communists (p. 120). Although differences between wars pointed up by Luard's categorization do exist, they are often not as great as the similarities that his categorization conceals.

In part this conceptual rigidity follows from Luard's explicit argument from analogy. He posits an international society, then asserts that such a society behaves just as other societies do, imparting values to its members. "The most important single thing society teaches its members," he asserts, "is what they should want; whether to value achievement or contentment, wealth or power, social success or artistic attainment, strength of arm or peace of mind" (p. 385). Having started with this premise, it is not surprising that he concludes "there is usually, as we have seen throughout this study, a considerable homogeneity in belief and behavior among all states within the same society" (p. 390). The current vogue of labels such as "trading states," "warrior states," and "crazy states" suggests that many scholars think otherwise.

Readers of encyclopedias have complained since the form was invented that while they may contain useful information for people who know little, they always get the details wrong in their own areas of expertise. Luard's book has the strengths and weaknesses of an encyclopedia—much information is assembled in handy form for the nonexpert, but it is unsatisfying to the expert or student desiring to become expert. Luard's own choice of the label *study* for his book suggests a past era, the solitary scholar alone in the library with pen and paper.

By contrast, Harkavy and Neuman have produced a very contemporary book, an assembly of papers generated by a conference of experts. Their effort is a superior one. The thematic essays all manage to address the subject of the book, saying something original while drawing on contemporary scholarly literature. The case studies illuminate the general theme. While no one would pretend that these studies are the last word on the particular conflicts, they are by and large comprehensive and complete.

Harkavy lays out the problems of comparative analysis, with ample reference to the literature. Starr and Most follow with a 20-page summary of war in the contemporary world that most readers will find more useful than the 450 pages of the Luard book. The remaining two introductory essays, by Luvaas on "lessons" in historical perspective and Dupuy on quantitative predictors of outcomes of wars, are more tangential but worthwhile in their own right.

The eight cases studies vary in quality, from a fragmentary look at the Falklands War (1982) to thorough accounts of the China-Vietnam (1979) and Israel-Syria (1982) conflicts. Chapters on Ethiopia-Somalia (1977-78) and Morocco-Polisario (1975 on), Soviet Union-Afghanistan (1979 on), and various conflicts in Central America (1972-84) provides as much information as the nonregional specialist might care to absorb. Only the chapter on Iran-Iraq suffers from being seriously out of date.

The work coheres well enough that useful lessons can be drawn, some obvious, some not. As Neuman points out in her concluding chapter, preponderance in numbers or equipment does not ensure victory: strategy and tactics count for much. Equally important is mastery of logistics—these wars have been expensive. Partly for this reason, conflicts have been increasingly internationalized. But Western participation has declined, while involvement of other third-world states has increased. Further analysis is promised in a second volume.

DAVID W. ZIEGLER

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The Soviet Study of International Relations. By Allen Lynch (Cambridge: Cambridge University Press, 1987. xii, 197p. $34.50).

Western, particularly U.S., international relations scholars tend to ignore the works of their Soviet counterparts. They have assumed a slavish Soviet adherence to the Marxist-Leninist "vertical, class-oriented paradigm" in which horizontal relationships (as those between nation-states) are seen as derivative from the vertical interclass relationship.

The Soviet Study of International Relations reveals this assumption as untrue. Its author, Allen Lynch, is the deputy director of studies at the Institute for East-West Security Studies in New York. Lynch tests a hypothesis that Soviet international relations theory (characterized by one Soviet scholar as "creative Leninism") has moved substantially in the direction of theory emanating from the West (*convergence* is a term that the Soviets reject). Lynch's book is, in effect, an extention upon William Zimmerman's treatment of trends in the Khrushchev and early Brezhnev periods (*Soviet Perspectives on International Relations, 1956-1967,* [1971]). Zimmerman found that Soviet scholars (indebted to the groundbreaking Stalin era works of Yevgeny S. Varga) were coming to recognize the primacy of states over classes as key elements of international relations, the primacy of the political sphere ("superstructure") over the economic (the "base") in interstate relations, and the significance of the structure of international relations ("system").

Lynch confirms that characteristics and tendencies revealed by Zimmerman have been sustained in the ensuring years. Lynch's book may be considered the definitive assessment of the state of the Soviet international relations discipline as of the early Gorbachev era.

Among many trends and patterns that Lynch reveals in his review of recent and contemporary Soviet international relations literature are

1. acceptance of the primacy of politics over economics as the essential stuff of international relations and a focus upon states as the most central elements; increasing perceptual differentiation between domestic and international political phenomena, particularly in the assumed centrality of class struggle to the former in contrast to the latter
2. the development and refinement of Soviet global systems theory; some corollary recognition of the insights of Western theorists such as Morton Kaplan, Charles McClelland, and Stanley Hoffmann.
3. "deutopianization" of Soviet international relations perspectives, owing to recognition (in implicit repudiation of Leninist orthodoxy) of the indeterminacy of outcomes, catastrophic potential of nuclear war, and "underlying stability" of the West
4. a preoccupation with systemic bipolar configurations; that is, the "socialist" and "imperialist" camps as critical subsystems and particularly their two leading actors, the Soviet Union and United States; recognition of considerable United States advantage, despite the passing of U.S. "hegemony."
5. recognition of tendencies toward systemic multipolarity—understandably, an emphasis upon "contradictions" (particularly those involving U.S. relationships with Western Europe and Japan) within the "imperialist" bloc more than upon differences among "socialist" states (except the People's Republic

of China, which the Soviets continue to characterize as "socialist," but have come to recognize as supplanting the United States as the principal external threat); admission, however, of the stability of the "imperialist" bloc, despite contemporary "crises" of capitalism.

6. Attention to "scientific-technical revolution," with a sense of uncertainty as to its outcomes vis-à-vis the Soviet position in world affairs.

In deference to ideology, Soviet theorists criticize the "realist" perspective in Western international relations scholarship. Lynch contends, however, that in its conceptual separation of "superstructure" from "base" (and perhaps in the consignment, by Soviet scholars, of international organization to the periphery of international relations), Soviet theory has incorporated much of the spirit of "political realism."

But normative elements remain as salient features of Soviet theory. This is indicated in the hortatory language in which it is articulated and in Soviet *appraisals* of the present system as fundamentally illegitimate. As to this world order, Soviet theory grants to Soviet citizens the prerogative (even obligation) to explore "ways of bringing about its demise, so long as this does not threaten the hard won achievements of socialism" (p. 147). Soviet international relations theory, divested of orthodoxy, has moved in the direction of theory in the West. But in this normative sense, it remains distinctly Leninist, and full convergence is most unlikely in the foreseeable future.

Readers with a broad understanding of Leninism, Soviet politics, and contemporary Western theories of international relations will find Lynch's synopsis of this Soviet literature to be both valuable and comprehensible. His extensive bibliography will be useful to those who have mastered the Russian language and have access to Soviet scholarly works. Lynch alludes to an increasing autonomy in the Soviet study of international relations. He means by this growing recognition of it as a field of study distinct from the other behavioral sciences. This field has *not* been separated from regime or party. Thus its theorists, more than their Western counterparts, instruct decision makers as to world perspective and the development of policy. Western counterparts of these Soviet decision makers should, therefore, find particular value in Lynch's book.

J. DAVID GILLESPIE

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Africa and the Middle East Conflict. By Arye Oded (Boulder: Lynne Rienner, 1987. xii, 244p. $29.50).

Ayre Oded provides a broad and well-researched survey of African relations with Israel and the Arab world since the 1973 war. It relies primarily on extensive use of newspaper and magazine sources, including many African and Arab publications. Although the preface refers to interviews and meetings in Africa there is no direct reference to these materials in the book.

The thesis is that since 1973 Arab states have achieved impressive gains in cultivating Africa through economic aid, diplomacy, use of Islam, and other instruments. Persistent problems plague African–Arab relations, including the legacy of the slave trade, African disillusionment with the nature and extent of Arab economic aid, fears of Islamic fundamentalism, and struggles over leadership in the Organization of African Unity and other entities. However, since 1973 only Zaire, Liberia, Cameroon, and the Ivory Coast have restored full diplomatic relations with Israel, in addition to Lesotho, Swaziland, and Malawi, which never broke relations. In spite of the Camp David accords, Arab pressures and assistance have maintained the facade of Arab–African solidarity on Arab–Israeli issues.

While the brunt of the book examines Arab–African relations, two chapters deal with Israel and Africa since 1973. These are particularly disappointing given the author's access to official Israeli sources. Three superficial pages discuss Israeli–South African concerns about alleged clandestine contacts. There is no mention of the Israeli rescue efforts on behalf of Ethiopian Jews and the impacts these had elsewhere in Africa. We are told that Israel maintains extensive commercial and technical assistance ties with states with whom it has no diplomatic relations (Ghana and Kenya) but are given little insight into how those ties operate and why.

The material on Arab efforts to influence Africa is useful but highly uneven. An important chapter discusses official Islamic organizations and their activities but is bereft of any sense of how Islam in black Africa operates or of references to the literature. The maze of promised and actual Arab multilateral and bilateral aid to Africa is adequately sketched. Two chapters on African-Arab relations rely unduly on scattered editorials and letters to the editor from the African press and lack context and substance. It may be that more systematic data such as in-depth interviews and public opinion polls is lacking, but what is presented is too prone to selective bias by the author.

The conclusion is that since 1973 "the balance of African-Middle Eastern relations has definitely tilted in favor of the Arabs" (p. 222). Israel has managed to maintain working relations with a handful of states but it has been unable to convince more than a few states to resume diplomatic relations. This is attributed largely to the pressure of possible Arab reprisals, although the author notes that the four states that have restored relations since 1982 have not experienced significant reprisals.

What is lacking in this informative but limited study is an analysis of African, Arab, and Israeli interests in one another as perceived, implemented, and altered by decision makers, and where relevant, public opinions. Have the Arabs convinced most Africans that it is in their interests to support Arab views on the Arab-Israeli conflict in exchange for some economic aid and reciprocal support for African views on South Africa? Do African elites and publics share a sense of solidarity with the Palestinian cause or Arabs with that of black South Africans? What does Israel have to offer Africa, and vice-versa? This book is a useful effort to compile meaningful responses.

AARON SEGAL

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British and American Approaches to Intelligence. Edited by K. G. Robertson (New York: St. Martin's, 1987. x, 281p. $39.95).

The title of this often stimulating collection of essays is somewhat misleading. The clear emphasis is on British experience in modern intelligence policy and concepts. Ten of the 11 essays are by British authors who have either served in the Foreign Office or associated with British universities and publishing houses. While a few examples of U.S. intelligence policy are cited, there is no real effort made to integrate U.S. ideas into comparative framework for analysis. The unfortunate title aside, the collection as a whole contains some worthwhile material.

The stated theme of the collection is threefold: the nature of intelligence, the development of British intelligence, and the relationship between intelligence and democracy. The essays that are rather loosely grouped under these headings do not begin to adequately address the broad topics suggested, but they do make a number of fresh contributions.

John Lockhart served 20 years with the British Foreign Office (1945–65). His "Intelligence: A British View" is written with an understated wit and common sense that most Englishmen would like to think is the hallmark of that island race. His concluding "Observations on Intelligence" is a sort of Murphy's Law of aphorisms on what can go wrong with intelligence operations in a democratic country; they should be memorized by everyone in the intelligence services from top to bottom.

"The best and most interesting essay is that of Wesley Wark, "Intelligence Prediction and Strategic Surprise: Reflections on the British Experience of the 1930's." Essentially, this essay is a distillation of his longer work, *The Ultimate Enemy*. His thesis is simple but rich in implications. Most of the literature on intelligence failures in World War II have followed the "communications model" pioneered by Wohlstetter's study of Pearl Harbor. Her argument was that the inability to separate background noise from true signals tends to bias intelligence analysis in favor of preconceived ideas. But as Wark points out, this usually assumes the existence of a set of convictions about potential enemies. The threat of Nazi Germany was unique. It was the first of the totalitarian states to pose a military threat to liberal democracies. The background noises were traditional assumptions about state behavior and the real signals were the unique ideological patterns of the Nazi state. While much of the thesis itself is not entirely new, Wark does successfully place it in the context of intelligence analysis in a way that makes intelligence more than a policy issue, and helps focus attention on intelligence as a philosophical question as well.

The weakest part of the collection is "Intelligence and Democratic Values." The general subject speaks of its own importance, but the specific problems are far less obvious. Given the nature of the subject it might have been better to omit this section altogether than to deal with the questions so inadequately. The essay by Eunan O'Halpin is a fine study of how British intelligence was financed from the turn of the century through World War II, but it doesn't seem to fit here. Michael Supperstone has contributed a good essay on the law relating to security in Great Britain, but doesn't pretend to exhaust the issue. The final essay by K. G. Robertson attempts to compare British and U.S. attitudes toward society in intelligence, but while he casts his conceptual net far enough to encompass most of the pertinent questions, the short essay format allows his subject to escape through the numerous holes. Taken as a whole, the reader wishing to explore intelligence and democratic values will have to look elsewhere.

The weaknesses of this collection are not due so much to the quality of the essays contained therein. It is rather a weakness born of collecting a series of conference papers into a published volume. The imposition of order on case studies and otherwise eclectic material is likely to be arbitrary at best and superficial at worst. This collection generally fits that pattern. Having said that, however, it must also be said that the essays themselves are generally of high quality and several are quite stimulating. Students of intelligence operations will find a few new morsels to chew over and newcomers will find some genuine food for thought.

SIDNEY A. PEARSON, JR.

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The Origins of Alliances. By Stephen M. Walt (Ithaca: Cornell University Press, 1987. x, 321p. $32.50).

Notwithstanding the amount of attention alliances have received in the literature of international relations, it is fair to say that our comprehension of them is still imperfect. One unresolved problem can be found in our fragmentary understanding of the manner in which nations choose particular alliance partners. This book is devoted to that problem.

In order to understand how alliance partners are chosen Walt articulates three sets of hypotheses. The first of these revolves around the question of whether nations choose alliance partners to balance against power or jump on the bandwagon with it. The second relates to the extent to which ideological similarity between governments influences specific alliance choices. Finally, Walt seeks to uncover whether foreign aid and political penetration cause alignment choices. The main value of the three chapters is not their originality, for, as the exhaustive notes tell us, most the substantive material has been presented by others before. Rather the value is to be found in the enviable manner in which Walt pulls together a vast amoung of literature and summarizes the essential arguments into a relatively few hypotheses.

To evaluate the accuracy of the hypotheses, Walt then presents two chapters reporting the intricate twists and turns of military-diplomatic events in the Middle East between the Baghdad Pact and the Camp David agreements.

The main conclusions that follow from the comparison of the hypotheses with the historical narrative may be readily summarized. First, in general, nations choose to enter alliances for the purpose of balance against a nation that is threatening. Put differently, there is little evidence of the bandwagon and less that nations simply react to power by itself; the element of threat usually needs to be present to activate balancing alliance activity. Secondly, Walt argues that the overall pattern of alliance activity in the Middle East "offers only modest support for the hypothesis that states with similar domestic systems are more likely to ally with one another" (p. 214). Finally, with respect to the influence of economic and political penetration, Walt finds that these instruments are not likely to be effective in themselves but that when the interests of nations converge such things as aid and arms transfers may reinforce existing interests and reduce the friction of the arrangement.

The final chapter casts the three sets of findings against a general discussion of alliances and a consideration of the relationship between the U.S. and Soviet alliance systems as they now function. Although the broad discussion of alliances in this chapter is not closely tied to the set of theoretical questions that served as the initial point of departure for the study, the policy recommendations offered in the final pages are nonetheless satisfying because they follow from a general set of propositions much more than is normally the case in policy analysis.

In sum, the book provides an interesting discussion of three possible factors in alliance formation; an excellent brief summary of post-World War II Middle Eastern diplomacy; and a thoughtful, theoretically driven set of policy recommendations.

That said, it must be admitted that the study is not without its problems. While there are some minor points of difficulty (such as often-mistaken discussions of rational choice theories of alliance formation), the major problem of the work is its failure to maintain the high theoretical level that guides its initial purpose. Instead of explicitly stating a set of Waltzian assumptions about the *necessity* of alliances in the international system and then allowing these assumptions to interact across considerations of national power, ideology, and penetration, the author turns to a consideration of the three individual factors in the formation of alliances without a well-formed consideration of how the constraints and imperatives of the international system interact with the three identified factors. In addition, the analysis of the case data is carried out within a research design that simply will not permit a sufficiently rigorous framework for the purpose. For example, is it reasonable to expect that the range of difference in power, ideology, and penetration in the Middle East over a relatively short period is sufficient to adequately probe the hypotheses? While Walt asserts that he is using the technique of focused comparison, the absence of other distinct cases to add to the variation in the independent variables detracts from the "tests." The adoption of a more rigorous approach to the coherence of the theory and domain of the research design might have led to a discussion of the role of national autonomy and policy preferences in alliance choices. As it is, these factors are only implicitly considered.

Admirable as some aspects of this book are, its difficulties should remind us of how far we have to go in designing research to address the problems that drive inquiry in international politics. It is, of course, the case that fully satisfactory research designs are difficult to devise because of the difficulty of establishing statistical controls on the small data sets usually encountered in the field, but this realization should lead us to specify our theories more closely.

RANDOLPH M. SIVERSON

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Egypt and the Politics of U.S. Economic Aid. By Marvin G. Weinbaum (Boulder: Westview, 1986. xi, 192p. $23.00).

Egypt was once called the gift of the Nile. Today things have changed considerably. Since the construction of the High Dam at Aswan at the border between Egypt and the Sudan a generation ago, the river that for centuries brought nourishment to Egypt's parched soil has lost much of its life-giving quality. The very waterway that once afforded unimpeded passage across the entire length of Egypt to innumerable dhows and other rivercraft is today all but choked over much of its course by an innundation of tenacious weeds.

Where Egypt was once the breadbasket of the Middle East, today it is dependent upon economic help in vast amounts from overseas. This help is needed not only for economic development but to sustain the people of the Nile valley with the necessities of daily living. Since the mid-1970s the United States has supplanted the Soviet Union as Egypt's principal benefactor. Today, Egypt has achieved the distinction of being second only to Israel as the main recipient of U.S. economic and military aid.

Marvin Weinbaum's small book, *Egypt and the Politics of U.S. Economic Aid*, sets out to examine how both Egypt and the United States have gained from the vast expenditure of funds. While Weinbaum puts the best possible gloss on his account, the story is not a happy one, regardless of whether the perspective starts from Washington, D.C. or from Cairo.

Weinbaum shows in considerable detail the many ways in which the billions of dollars expended on economic and military aid have not only repeatedly fallen short of their declared objectives but failed, in the bargain, to elevate the United States in the esteem of the Egyptians. Even worse, the gift giver is increasingly blamed by the Egyptian populace for the aid program's inability to lift Egypt out of its chronic despondency. Nonetheless Weinbaum seeks to console the U.S. reader with the judgement that U.S. assistance has, for all its shortcomings, served to sustain Egypt in the difficulties it has endured at the hands of its fellow Arab states ever since the Egyptian-Israeli peace treaty was signed in 1979.

In his concluding sections, Weinbaum suggests that the U.S. might obtain better Egyptian compliance with the entire aid program and thereby make it more effective if the United States adopted a sterner stand whenever Israel is held by Arab states to cause them offense. Curiously, Weinbaum never considers the benefit that the U.S. brought to Egypt itself by bringing the peace treaty to completion and thereby releasing Egyptian military forces to confront their potential enemies on their African frontiers: Libya, Sudan, and Ethiopia.

So far, both Egypt and the United States have muddled through. But the era now upon us, where each U.S. budget dollar is being scrutinized closely for its program effectiveness, means that the time is fast approaching when the U.S. assistance program in Egypt will be examined more closely than before. It is a pity that Weinbaum does not provide much guidance for that upcoming undertaking.

STANLEY NEWMAN

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POLITICAL ECONOMY

Multinationals, the State, and Control of the Nigerian Economy. By Thomas J. Biersteker (Princeton: Princeton University Press, 1987. xvii, 344p. $45.00, cloth; $12.50 paper).

In the field of the comparative political economy of development, no question is more compelling and elusive than that of control. The issue of who really controls the economies of developing countries—their own states, local bourgeoisies, or the multinational enterprises that invest directly in them—lies at the core of the ongoing debate between various neoclassical and radical (dependency or neo-Marxist) schools of theory. Few studies have examined the issue as seriously, imaginatively, and productively as has this impressive new book by Thomas Biersteker.

Multinationals, the State, and Control of the Nigerian Economy is one of the first works on the political economy of development to penetrate the process by which third-world states make and implement policies on foreign investment. Thus whereas most previous works have been able to draw only weak inferences about the (changing) nature and balance of power relationships, Biersteker derives a number of well-supported conclusions from a rich and original array of data sources—in-depth interviews with 157 state officials, multinational managers, indigenous businessmen, and other key actors or observers; government documents and statistics; academic and policy papers; and an elaborately coded set of data on Nigerian corporations.

Such close study of the policy process is the only way of determining anything conclusive about the structure of power. That this behaviorist method yields compelling but qualified support for some predictions of dependency theory is one of the many refreshing and provocative features of this work.

The book examines the origins and determinants of Nigeria's two indigenization decrees (1972 and 1977) and then assesses their consequences for control of the Nigerian economy at three levels: individual firms, economic sectors, and the Nigerian economy as a whole. The author begins "by identifying the actors with effective channels of influence in each issue area, and then attempt[ing] to determine their principal objectives, concerns, and disputes" (p. 290). He proceeds to disaggregate the major corporate actors (the state and foreign and local capital), looking for contending interests and motives within each.

The analysis of the two indigenization decrees shows how each key group pursued its interests and pressed, influenced, or checked the others and thus how each decree represented a compromise of some sort—the product of a distinctly *political*, rather than rational, process. The state can mediate, as it did for the formulation of the first decree, or it can initiate and lead, as it did for the second; but no single corporate actor can simply impose its will. Also examined is the process of policy implementation, both the actual functioning of state institutions and the evasive, "counter-offensive" behavior of multinational enterprises. The potentially contradictory nature of the formulation and implementation phases of the policy process is an important methodological lesson of the study.

Theoretically, the book contributes an original, dispassionate, and remarkably clarifying dissection of six contending perspectives, which demonstrates inadequacies in every one. Thus, the author's approach is deliberately eclectic, seeking to evaluate and draw from the contending theories. Admirably, Biersteker resists the temptation to reach for sweeping generalizations. Having no theoretical axe to grind, he recognizes that the empirical world is messy, contradictory, and full of nuance and that caution is in order when generalizing from a single, distinctive case.

The key general finding is the complex one that "there is no single theoretical explanation for the sources of programs of this sort (e.g., capital does not always capture the state and the state does not always have the capacity for autonomy)" (p. 290). There will also be keen interest in his conclusion that the shift of bargaining power from multinational enterprises to host countries is neither stable nor inexorable but part of a mutual process of learning, adaptation, and struggle for control.

With respect to the issue of control, Biersteker concludes that there has been virtually no change yet at the level of the firm, but he does uncover (and is the first to do so) some potentially historic developments. There has begun to be some significant indigenization of personnel at the middle and senior ranks. Due not simply to the decrees but to the active managerial intervention of the central bank, Nigeria has taken control of its own financial sector. Most interesting, perhaps, the book reveals some movement of the traditionally comprador, commercial Nigerian bourgeoisie into productive activity, as the indigenization decrees have opened a possible niche for local capital in low-technology manufacturing.

The book is not without its flaws and limits. Although there is a note of caution (p. 246), the author has not done enough to warn the reader (and perhaps himself) of the questionable basis for drawing conclusions from his data set of 1,230 firms (only about half of the 2,380 incorporated in Nigeria at the end of 1980). I would have preferred to see more treatment of the movement of Nigerian capital not just into manufacturing but possibly even more striking, into agricultural production in recent years. More attention should have been paid to the most nagging (albeit most inscrutable) element in the whole problem of economic control and public policy in Nigeria—corruption. There could also have been a more sustained analysis of the role of bureaucratic leadership, which gets some fascinating but abbreviated attention, and of the fate of implementation after the transition back to civilian, democratic (and more corrupt) rule in 1979.

However, no one study can do everything. To be sure, this book's creativity of design, balance of perspective, rigor of method, depth of evidence, and wealth of richly documented findings mark it as one of the most important contributions to the field in many years.

LARRY DIAMOND

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Rivalry: In Business, Science, among Nations. By Reuven Brenner (New York: Cambridge University Press, 1987. xi, 244p. $27.95).

The world is in the midst of dizzying changes in both product and process technologies. Computers that two decades ago measured their speed in dozens of operations per second now carry out more than 20 thousand arithmetic functions in that same time. Automobiles that once took 80 months or more to move from the design board to production now appear in less than half that time.

New medical technologies that used to take a dozen years to develop now move to their final testing stages in a matter of months.

These changes in technology—the ways that things are done—are reshaping the world economic arena. Japan has become the world's preeminent economic power by harnessing its traditional predilection for new technologies to the cart of economic growth. In Europe, Germany's economic growth is based on its continuing ability to transform technological developments into useful products at a startling rate.

Why does such a harnessing of technological development to economic growth occur in some societies and not in others? A spate of analysts have addressed this question by focusing on the role of the state and by examining the policies that government pursues in promoting economic change and growth. The Japanese miracle, for example, is often explained in terms of its government's fabled industrial policies; China's growth over the past decade is described in terms of its government's Open Door policies of the late 1970s.

But just how important *is* government's role in pushing economic development and technological change? Reuven Brenner, in *Rivalry: In Business, Science, among Nations,* examines some of the theories that point to the increasing role of government in shaping the nature and pace of technological change.

Brenner takes exception to many of the conventional wisdoms that underlie much of the current analysis. Ultimately, his arguments rest on one important proposition: it is not government, he asserts, that is primarily responsible for the process by which new technologies are acquired and implemented. The proper focus, he argues, ought to be the building blocks that constitute the core of the modern economic system—the individual enterprise.

Brenner begins by questioning the concepts of *market* and asking what it is that enterprises set out to do. Classical economic theorists often argued that an enterprise was simply an organization for "profit maximization." Closer to reality, argues Brenner, is a definition that views an enterprise as a collection of individuals who are motivated (or inhibited) in their search for and acquisition of new technologies.

His early chapters outline some of the players in the technology acquisition process. Echoing J. B. Say at the end of the eighteenth century, Brenner focuses on the role of enterprise leadership—especially leaders who are "entrepreneurial." An entrepreneur is the "individual who bets on new ideas and implements them" (p. 45). The key variables here are judgment, vision, and insight into people. Entrepreneurial managers focus on innovation and the individuals who might push an enterprise toward innovation rather than financial profit-and-loss criteria.

In a rapidly changing world, Brenner argues, the key to the competitive struggle lies in the enterprise's ability to match customers with services. As customers' needs change, innovative firms with entrepreneurial leadership "leapfrog" into new product and process technologies. Competition between firms, he states, is not so much concerned with a struggle for comparative advantage in pricing as it is with the battle to meet new needs.

Large firms and cartels represent a special danger to the course of change. The danger arises not so much from the oft-observed tendency of cartels to control price, according to Brenner. The real threat from monopolies and cartels, he argues, is that they often inhibit the process of technological change.

How, then, is the wealth of nations maintained or restored? The key, Brenner concludes, lies not simply in the formulation of government "industrial policy" or in the maintenance of a system of "free trade." More important, he argues, is the ability of a society to maintain the creativity of those who produce material goods and new technologies—those within the enterprise and the entrepreneurs behind the enterprise.

Unfortunately, this book is a tough read. Quite often the arguments bog down in difficult language, awkward syntax, and hyperbolic generalization. The editing seems incomplete, and I wish that the author had written at least one more draft.

Nonetheless, Brenner's is an important argument, and he elaborates the argument with some interesting case studies. Moreover, the argument is much more than a simple paean to unfettered laissez-faire capitalism. Command and market economies alike, this argument concludes, must all do more than simply devise attractive macrolevel policies. The real trick is to motivate the actors who are the key players in the game of economic change—the discrete units that make up the microlevels of any society.

ROY F. GROW

Carleton College

Peasants against the State: The Politics of Market Control in Bugisu, Uganda, 1900–1983. By Stephen G. Bunker (Urbana: University of Illinois Press, 1987. 296p. $27.50).

The study of the contemporary African political economy is in ferment. From the rather simplistic expectation in the 1950s and 1960s that the Western liberal-democratic model would guide and explain African politics to the fascination of the late 1960s and 1970s with the seemingly militant single-party mobilization to the attempts to explain all African political processes as extensions of world dependency or class conflict models, African political specialists have tended to overreact to events in the capital and over-apply political-economic models generated in other areas, under other conditions. The lack of fit between the worlds that these models predicted and evolving events, along with the rather rapid about-faces taken in choosing the models, have been clues that something was seriously wrong. Even the attention paid to the "soft state" concept by current analysts leaves some significant ambiguities: How can it be sensible to talk of a "soft state" and overcentralization at the same time? Are the brutal dictatorships which arise from time to time examples of "soft states," reactions to them, or aberrations? How is order maintained in the "soft state," and what sorts of relationships exist among major political actors in them? How are they related to Africa's contemporary economic problems: Are they a cause, a result, or part of a solution?

As a result of the poverty of most theorizing done hitherto on the African political economy, the very appropriateness of conventional social scientific concepts commonly applied to Africa needs to be questioned. *State, representation, peasants, ethnicity, bureaucracy, administration,* and other terms have historically evoked a host of images and implications out of a largely Western experience, which current (and selected earlier) research (Donald Rothchild, Nelson Kasfir, Goran Hyden, Polly Hill, Sara Berry, and Audrey Smock) increasingly shows simply does not fit into the reality of contemporary Africa.

In a finely researched, well-written and theoretically sophisticated field study, Stephen G. Bunker earns a place in this selected list as he contributes substantially to our understanding of these concepts and how they must be applied to Africa.

Bunker's focus is on smallholder, coffee-producer organizations formed to protect their economic interests in the richest coffee-growing area (Bugisu) of Uganda. In a detailed history of the period 1900–83, Bunker reviews the interactions between the Bagisu (people of Bugisu) and central authorities, the British, the Uganda independent government, and the postcoup regime of Idi Amin. In the first two cases he shows how the smallholder producers were able to utilize their control over production to mediate, negotiate, and modify central policies and programs to better fit Bagisu interests. The independent base of the freeholders along with the state's need for their product meant they had leverage, which they were quick and able to use to protect their interests. Only in the ruthless random violence of the Amin regime, where the base of the state was essentially no longer production but brigandage, did the Bagisu lack leverage.

Bunker's study is instructive in several regards. The rather simple dynamics of dependency theory are strongly challenged by his study, as smallholder producers were clearly *not* "captured" and "molded" by "international capital" but used their capacity to organize and control the production process to protect their economic, social, and political interests. Similarly, the Ugandan state (like the British colonial government) was not able to dominate the Bagisu, but had to negotiate with them and develop exchange relationships where profits and development flowed up-country in return for a steady and substantial flow of the commodity. A critically important insight, furthermore, is that this moderate, limited state was perhaps an optimal state for all concerned. This is because the drive of the state to extract ever additional capital from agriculture is not controlled by any dynamics internal to the central state. *If* unconfined, it will squeeze the smallholder until he ceases production, and the state will collapse economically. The "strong" state becomes the impotent state, as recent history in Ghana and Tanzania suggest. Thus a state forced to negotiate with its citizens may be, in the long run, a more powerful entity than one seeking dominance.

The important role played by political intermediaries such as local administrators, party and cooperative officials, and members of parliament, is also demonstrated here. These officials act as the "balancing function" between state and small producer. They must retain the confidence of both to maintain their economically privileged status, and to do so they must extract enough wealth from the producers to sustain the state but strike a good enough deal for the producers to keep them growing cash crops. The high priority on dominating this political space played by militant regimes such as those of Nyerere, Nkrumah, and Toure can thus be seen to have damaged or destroyed a critical resource for the state.

Bunker thus shows us that smallholders are not passive subjects unable to affect the world they work and live in; dependency dynamics and class analysis do not, at least in this case, explain domestic political-economic relationships in Africa in any simple fashion; "strong" or militant states are in effect "soft" states (ineffective) in their ability to actually *reach* such goals as economic development; the unconstrained coercive power of the state is a blunt, ineffective, and ultimately destructive tool of social organization; nominally centralized governments are dependent on independent and decentralized social and political structures for management and organization of key sections of the economy; and states that allow (or accept) existence of political space for their citizens to negotiate "win-win" arrangements are more effective development agents than those which demand control and conformity. It is through detailed, careful, theoretically sophisticated but microbased field studies such as this one that our understanding of African politics and the appropriate concepts to use with it and our ability to contribute to resolving current problems will improve.

JAMES S. WUNSCH

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Futures for the Welfare State. Edited by Norman Furniss (Bloomington: Indiana University Press, 1986. vi, 444p. $22.50).

In the midst of continuing political and intellectual controversy concerning the viability of established welfare systems under recurrent conditions of international economic instability, this volume of original essays stands out as a thoughtful and informative contribution. The various authors address important issues of interest to comparative scholars and U.S. specialists alike. Their focuses and analytical methods differ widely, but they affirm in common the historical and contemporary significance of welfare institutions and provisions as ameliorative features of modern capitalism.

The book consists of 13 chapters, approximately half of which were originally presented as papers at a conference held at Indiana University in 1983. A total of 21 U.S. and European scholars collaborated to produce the final product.

Substantively, the various essays address three broad sets of topics, each of which corresponds to a major section of the volume. Those in part 1 deal with general conceptual and methodological questions pertaining to the analysis of the welfare state; essays in part 2 assess empirical trends in a number of advanced welfare countries, with particular emphasis on Scandinavia; and chapters in part 3 focus on the U.S. experience. Unifying the essays is a shared belief among the contributors that the established welfare systems of Western Europe and North America are the product of diverse political, economic, and social factors and that, within limits, their futures remain open to conscious political choice among key policy actors.

This thesis argument is forcibly advanced by Timothy Tilton in an opening chapter explicating key research problems pertaining to the welfare state. On the basis of a critical assessment of alternative theories of welfare enactment and retrenchment, Tilton concludes that industrialization, capitalism, and other ostensibly deterministic factors do not provide a sufficient explanation for welfare state development and performance. A more convincing explanation must begin, instead, with recognition of the institutional capacity of the state for autonomous action. Even more important, he adds (affirming the view of

Walter Korpi and others), is the nature of the prevailing balance of political forces in a given society. Thus, whether or not slower rates of economic growth and increased budgetary deficits will result in a reduction of welfare provisions depends on the composition and values of a nation's governing coalition.

Tilton's emphasis on the centrality of politics is underscored in a number of other essays, notably those by Douglas Ashford, John Logue, and Uriel Rosenthal. In a penetrating if sometimes murky analysis, Ashford concentrates on the role of high-level civil servants in the historical development of the welfare state in Britain and France. He argues, in the British case, that the prior existence of a centralized and bureaucratically efficient system of cabinet government resulted in the emergence of considerable institutional fragmentation and policy ineffectiveness, whereas in the case of the French a more adaptive and innovative national civil service achieved a more responsive array of policy instruments and programs. Ashford's larger point—which is eminently worthy of more extensive empirical analysis than is possible in a single chapter—is that contrasting institutional choices have resulted in significant national differences in policy cohesion and provision among the Western democracies.

Logue's chapter, in the second part of the book, concentrates on more contemporary facets of welfare state development. His focus is Denmark and Sweden, where the very success of the welfare state, he contends, has generated serious social and political problems. In the former instance, "the solidaristic values of the generation that built the welfare state are giving way to more egotistical values in the generation that grew up with it" (p. 268). As evidence he cites (without providing empirical data) an increased tendency on the part of citizens to utilize welfare services while simultaneously seeking to minimize the personal cost of such services through various legal means of tax avoidance. Politically, the formidable size of the public sector in Scandinavia, and ideological constraints on the part of both the Social Democrats and the non-socialist parties, are key factors that seriously restrict public efforts to undertake significant retrenchment measures in response to economic and fiscal crises. Logue's best hope for the welfare state is "general constraint" among its beneficiaries—to be achieved, if necessary, through government manipulation of individual incentives via changes in the tax system for example.

A third chapter worthy of note is Uriel Rosenthal's spirited effort in the final section of the volume to promote greater conceptual clarity about the relation between welfare society and the welfare state. Any attempt to theorize about the contemporary and future role of the welfare state, he argues, must build on this distinction while simultaneously taking into consideration configurative psychological and historical factors.

Furniss echoes Rosenthal's emphasis on the dichotomy between the state and civil society in a concluding summary essay. Noting that the relationship between society and the state in advanced capitalist democracies is "essentially adversarial and unstable" (p. 389), Furniss restates the view of Tilton, Ashford, and others that the welfare state emerged as a result of purposeful political action. To the extent that the modern welfare state has succeeded in regulating conflict among competing social forces even under conditions of recurrent economic crises, apocalyptic predictions about its imminent demise are at best highly premature.

Other essays stand out because of their methodological or empirical rigor. Among them are chapters by Jerald Hage on techniques for delineating periods of welfare state development, Don Schwerin's argument that large public sectors do not necessarily undermine domestic economic efficiency, and an incisive analysis by Larry Griffin and Kevin Leicht of the "politicization" of welfare expenditures in the United States through the budgetary process. Also useful for reference purposes is a fairly comprehensive bibliographical essay on central themes of mainstream research on welfare-related principles and issues.

No brief assessment can adequately summarize and critique as diverse and complex a collection as this. As a whole, the volume is an impressive achievement, flawed only by minor irritants such as the omission of an index and occasional editorial lapses and printer's errors. The sum of the essays is a reasoned, conceptually innovative, and empirically rich overview of contrasting patterns of welfare state development and future potentials.

M. DONALD HANCOCK

Vanderbilt University

The State and the City. By Ted Robert Gurr and Desmond S. King (Chicago: University of Chicago Press, 1987. ix, 242p. $35.00, cloth; $14.95, paper).

Invisible to much of the profession, under the general rubric *political economy* intense ideological battles are being waged in the study of urban politics. A range of analysts with varying perspectives, including, but by no means limited to, neo-Marxists, neo-Weberians, neoconservatives, and pluralists, have turned to the study of urban political, economic, and social phenomena as a battleground for their diverse theories.

Gurr and King join the fray, taking what they call a "state-centered" approach to the study of change in local government. They argue that national and regional "states" have distinct interests in cities (the "local state"), and that the growth and decline of cities is as much the result of the incentives, policies, and actions of these "states" as they are of the social and economic trends stressed in other theoretical perspectives.

The book consists of two distinct sections, of widely varying quality. The first two chapters plus the conclusion serve as an excellent window into the ideological debates now raging. Using their state-centered perspective, Gurr and King review and criticize a variety of alternative theoretical perspectives, attempting to integrate them into their state-centered approach. For example, dismissing rational choice perspectives, which emphasize the bureaucratic desire for budget maximization or the politician's need for reelection, Gurr and King argue that such models emphasize "short-term" objectives, while the state-centered approach they advocate emphasizes long-term concerns for the pursuit of programs and the maintenance of patterns of power and authority. Their analysis stresses the "programmatic commitments" bureaucrats and politicians have to social and political objectives that transcend the short-term interests emphasized in rational choice models.

Similarly, the authors reject the emphasis on economic forces that underlies Marxist theories. Their state-centered approach emphasizes the interest of the state in reproducing and legitimizing *itself*, not the capitalist system. Gurr and King also reject the "economic determinism" they argue underlies Paul Peterson's relatively mainstream analysis in *City Limits.* While economic forces clearly affect the ability, capacity, and legitimacy of the "local state," the authors explore the limits imposed by the interests of the "national state."

In general, these chapters are provocative and serve as a valuable introduction into an area of intense scholarly conflict. Unfortunately, the other chapters of the book are much weaker. In chapters 3–5 the authors present an analysis of urban politics in the United States and Great Britain using their state-centered perspective. A truly comparative analysis is not presented. While the authors recognize that their comparative "approach" emphasizes differences rather than "equivalences" (p. 184), I found two radically different analyses and the points of direct comparison too few and far between.

A more serious problem is that the analysis of the U.S. situation is weak and unconvincing. In chapter 4, the authors make a fundamental error in their analysis of intergovernmental aid. Underlying their argument is the singular importance they attach to the programmatic interests of federal *bureaucrats* in allocating aid to various cities. While this argument makes sense from their state-centered perspective, it totally misses the complexity of the intergovernmental system in the United States, a system in which *politicians* in Congress play a major role in setting formulas for the allocation and distribution of federal aid.

Moreover the empirical analysis is unconvincing. Both the bivariate correlation analysis and the multivariate "dynamic" analysis produce weak results, and the enterprise is marred by high collinearity in the independent variables. While the authors call the empirical support for their state-centered model "modest" (p. 148), this is a generous assessment at best.

In the concluding chapter, the authors return to their strengths, discussing future developments in urban growth. The concern for identifying characteristic patterns of urban growth is common to much of the urban political economic analysis now being undertaken. Following in this tradition, Gurr and King identify four possible types of cities they believe are emerging: old industrial cities, new industrial cities, administration-service cities, and welfare cities. They summarize the economic, political, social, and technical forces producing these patterns of growth and identify the likely future for each type of city. The arguments presented in this chapter are important for understanding urban development and for understanding much of the genre of present urban political economic research.

In the late 1950s and early 1960s intense ideological conflicts between pluralists (spurred on by Dahl's work in New Haven) and elitists (following Hunter's work in Atlanta) eventually spilled over from the study of urban politics to the discipline at large. Whether or not the present debate will transcend its current boundaries remains to be seen. I believe one reason the debate might *not* cross over is that the level of technical analysis brought to bear is often not up to professional standards. In this regard, *The State and the City* acts as a perfect window into ongoing research on urban political economy: intense and sophisticated ideological and theoretical analysis coupled with weak empirical work.

MARK SCHNEIDER

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Governing the Economy. The Politics of State Intervention in Britain and France. By Peter Hall (New York: Oxford University Press, 1986. viii, 341p. $36.00, cloth; $14.95, paper).

In this cowinner of the 1987 Woodrow Wilson Foundation Award, Peter Hall offers an impressive diagnosis of the "British disease" and an explanation of how the French avoided it. In the process he proposes a style of policy analysis that focuses on the nature of institutions on both sides of the state-society divide. His emphasis on the interdependence of society and polity is reminiscent of systems theory, although he rejects structural functionalism for its assumptions regarding universal functions and system maintenance. Hall has pithy and perceptive comments on the neglect of institutions in several current styles of policy analysis. For example, on public choice he notes that "the notion of simple rationality is relatively meaningless as a description of motivation until it is given more content by a specification of the organizational conditions that identify the direction in which rational action is aimed and the means it is likely to apply" (p. 13). On state-centered theories that postulate state autonomy, he warns that "institutional networks affecting state action extend well into society, in such a way as to expose the state again to societal influences" (p. 17).

In Hall's judgment, Britain's relatively slow growth rates in the postwar period are not attributable primarily to government restrictions on the market but rather to inefficiencies grounded in the structure of the market itself. These inefficiencies, in turn, are the result of a historical experience of imperialism and early industrialization. The city's orientation toward overseas lending made it a powerful proponent of deflation. The British economy suffered from the absence of large investment banks of the sort that emerged to support late industrialization in Germany and survived to guide industrial modernization in that country. Britain's trade unions and employers associations appeared to be robust but ultimately proved incapable of imposing discipline on their members. The government found itself unable to reduce inefficiencies in the market through corporatist agreements, even on those rare occasions, as in the late 1960s, when it was inclined to do so. Moreover, state agencies were embedded in a "labyrinth of institutional relations" which inhibited their desire for change (p. 68). The powerful Bank of England, like the city, worried most about inflation, while the treasury, perceiving its task to be control of public expenditures, had little interest or expertise in industrial policy.

Following the path that Andrew Shonfield blazed over 20 years ago, Hall looks to French planning as an example of effective state intervention. Yet, more than Shonfield, he is aware that French planners were less important for the specific targets they proposed (which the government often ignored) than for their successful efforts at spreading the gospel of economic growth through modernization to businessmen, politicians, and bureaucrats. Again Hall explains state intervention in France essentially in terms of institutions. The comparative weakness of trade unions, coupled with the discredited status of employers associations after their affiliation with Vichy, offered unique opportunities for the planners.

In contrast to Britain, where state aid to industry for things like regional development had little effect on efficiency, French government officials used their resources to strengthen sectors—and specific firms—with growth potential.

Throughout the chapters on British and French economic policy, Hall offers useful insights into the complex interrelationships among historical experience, institutions, and ideas. And yet his book is unlikely to end the debate over industrial policy. His admiration for French *é' tatisme,* which runs through the chapters on British policy, is not fully supported by his own account of the French experience. He is aware that the plan had limited influence over policy even in its heyday, then was reduced to irrevelance or to a simple statement of the government's spending priorities after the oil crisis of 1974. He notes that the "national champions" policy of the 1960s produced gigantic failures like Plan Calcul, white elephants like the Concorde, and expensive public charges like the steel industry. The French economic miracle was indeed impressive, but was it primarily the work of state intervention? It coincided with rapid growth in most Western economies and was no more impressive than its Italian counterpart, which, some would argue, occurred as much despite as because of state intervention.

Were he writing today, Hall might be less pessimistic about the prospects for Thatcher's strategy of modernization through the market. He writes, "if the diagnosis of Britain's long-term economic decline offered in chapter one is correct, the Thatcher experiment does not augur well" (p. 132). In view of Britain's growth rate of over 5% in 1987—among the highest in the Western world—one might question Hall's pessimism but not necessarily his analysis. The changing structure of the British work force will make it difficult for unions to recover their strength of the 1960s and 1970s. As for the status quo mentality of civil servants and businessmen, is it not possible that the Thatcher government could be playing the same resocializing role Hall attributes to the Commission du Plan in France?

Although one may quibble with the author over particular interpretations, there is little doubt that this is an important book, one that demonstrates that policy can be understood only in the context of institutions and their historical development. It is a book that merits careful study and emulation.

JOHN S. AMBLER

Rice University

Neighborhood Justice in Capitalist Society. By Richard Hofrichter (Westport, CT: Greenwood, 1987. xxxiii, 192p. $35.00).

"Neighborhood justice" refers to a national judicial reform movement which in the last 15 years has established over 350 community mediation programs throughout the United States. Similar to the juvenile court and small claims court reforms initiated at the beginning of this century, the neighborhood justice movement is characteristic of judicial efforts to institute informal procedures, such as mediation, for resolving family and neighborhood disputes. Unlike earlier informal procedure reforms, however, neighborhood dispute resolution programs are the product of federal planning and nationally based institutions. In this book Hofrichter explores the politics of state-planned dispute resolution programs that appear as decentralized community entities.

This book offers a sophisticated analysis of the role of law in maintaining social order and responding to competing demands on the state. Hofrichter does not replicate the traditional approach to analyzing reform, an approach that often seeks to measure the success of failure of reform. Instead he is concerned with understanding the organization and politics of social regulation under capitalism. Neighborhood mediation is explained from an analysis of the capitalist state and hegemony. Building upon contemporary Marxist theories of the state, Hofrichter theorizes the conditions that produce neighborhood dispute resolution.

Hofrichter's thesis is that informalism is a new form of crisis management. There are several preliminary steps in his analysis and key among them is his theory of the informal state. Hofrichter describes a change in the form of state power and examines manifestations of it at the community level. He argues quite persuasively that "the community itself, its institutions and physical space become implicated in production and reproduction" of state power (p. 70). A new form of power, informalism, leads to an expansion of the state;

but it is not necessarily a strategy for obscuring state power. Although he argues that the informal state "appears as [a] nonstate, employing ideologies and practices from everyday life" (p. 71), Hofrichter also maintains that the flexibility of informalism is compatible with an "increasingly unpredictable social environment which results from capital expansion itself" (p. 44). He further situates the role of communities in a larger context of redefining state power by discussing a variety of examples of the informal state, such as community mental health and worker participation schemes.

The nature of order in a liberal society is not treated as merely an internal problem for legal institutions, but in Hofrichter's study the subject matter of neighborhood dispute resolution—minor criminal and civil disputes between people who know one another—"reflect the tensions of life as lived in capitalist society" (p. 10). Drawing on the work of Ira Katznelson and others who link citizens and workers to political interests, Hofrichter maintains that the neighborhood dispute resolution center is "a response to disorganize community challenges by demobilizing and absorbing demands at the individual level" (p. 22).

The book is a significant contribution to research on law and social control. The analysis resists a tendency in political science and sociology to reduce law to an instrument of state power and thereby view law solely as a social control mechanism. Hofrichter does not adopt a determinist theory of state power but instead explores the role of legitimacy and its pursuit in producing *indeterminacy*. The informal state, he argues, embodies "ideologies that integrate people into the social order" and "oppose instrumental accumulation rationalities." Furthermore, he argues that there is a "contradiction between the rationalization or instrumental reason necessary for capital expansion and the cultural forms of everyday life which generate different social rhythms and oppose technocratic logics" (p. 42).

With a theoretical grounding in Marxist crisis theory, Hofrichter poses the following question for his empirical study of mediation: How does the informal state, which "coordinates and plans the infrastructure of communities through new, undemocratic administrative institutions, expect to manage community life without jeopardizing its legitimacy?" (pp. 76–77). Although his theory has already provided an answer to this question, he does illustrate how the search for legitimacy produces internal contradictions for neighborhood dispute resolution. Hofrichter gathered data from observations of mediation sessions and from transcripts of the hearings. He found that mediation is organized around sets of structured rules that guide disputants toward particular kinds of agreements. Through his analysis of the hearing process he shows that mediators, who are lay citizens trained in mediation techniques of conflict resolution, are not passive participants but authoritative interveners. From these findings Hofrichter concludes that the organization and dynamics of the neighborhood dispute resolution process produce a legitimacy problem for the informal state. What was to be informal and flexible is formal and inflexible: "The rules are hegemonic because they restrict and direct the organization of the experience in the situation, while giving the appearance of something else" (p. 129).

Hofrichter has blended together a rich theoretical perspective with an original analysis of mediation hearings. This work provides a challenging new approach to studies of state power and the politics of law. It also takes us beyond the conventional focus of policy implementation studies by demonstrating the dynamics of legal procedures and the complexities of crisis management.

CHRISTINE B. HARRINGTON

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Congress: Structure and Policy. Edited by Mathew D. McCubbins and Terry Sullivan (Cambridge: Cambridge University Press, 1987. xii, 563p. $49.50, cloth; $16.95, paper).

Do policies make institutions, or do institutions make policies? The answer to this chicken-and-egg question turns upon one's research assumptions and objectives. The present collection, reflecting currently fashionable scholarly concerns that the editors term "new institutionalism," takes the latter point of departure.

The "modern era" of congressional research began when behavioralists, borrowing concepts and methods from sociology and anthropology, began to study Congress as a complex human organization. They emphasized leaders and followers, subgroups and factions, lines of communication, and especially norms and folkways. The result was a descriptively rich portrait of the post-World War II Congress. This system was transformed, first by wide-ranging structural reform and then by agenda shifts that brought sharp debates over policy content and priorities. Not by coincidence, political scientists turned to economic models to address a new question—how Congress's structure dictates certain decision sequences and policy forms.

Signaling this paradigm shift, the McCubbins-Sullivan collection asks (in the series editors' words), "How do institutions evolve in response to individual incentives, strategies, and choices; and how do institutions affect the performance of political and economic systems?" (p. ix). Compare this with the questions raised by a recent collection of a more behavioral flavor: "How are decisions made within the political system? How and why do institutions change?" (Glenn R. Parker, ed., *Studies of Congress* [1985], xi).

Comparing the McCubbins-Sullivan and Parker volumes is instructive because they have many things in common (length, quality of articles, lucidity of editors' essays, and even some of the contributors). The two have comparable sections on elections and such topics as parties, committees, and leadership (though Parker gives more attention to leadership). It is in the final sections that the two books part company. Whereas Parker's voting studies are about factions, coalitions, and constituent influences, McCubbins and Sullivan are more intrigued by theoretical models of voting choice. The payoff of the newer approach comes in the final section, not duplicated in the other work—the impact of institutional arrangements upon public policies.

The early Mayhew-Fiorina formulation has much prominence, although the editors suggest (pp. 14-15) that important modifications must be introduced to overcome some of its shortcomings. Jacobson's informative, sensible essay brings things up to date and fills in the gaps. Nonetheless, the impression conveyed by this literature is that elected officials are free to bend congressional structures and bureaucratic functions to their reelection needs. In fact, Congress is *not* "perfectly designed to serve the election needs of its members" (p. 73). Leaving aside members' nonelectoral goals, governmental structures respond to contextual factors and agendas that often vastly complicate office-holding tasks. If the 1970s didn't make this disjunction between individual incentives and institutional imperatives obvious, the 1980s have dramatically done so.

Members' complex motives come into play in the institutional pieces—for example, Fenno's comparative analysis of House committees. It is hard to overstate the importance of committees, but the autonomy of committees as decision units may be partly an artifact of the pre-1970s era. Committee boundaries are more easily overstepped in the Senate, but the House is following suit. At a time when more than a quarter of the average House committee's workload is shared by one or more other panels, we need to rethink our assumptions about committee operations.

The early sections of the book, mostly conventional in content, serve as foundation for the editors' ultimate concern: how legislative decision rules mandate consistent patterns of policy. There is a lucid introductory essay by Thomas Schwartz, along with important articles by Shepsle, Ferejohn, Arnold, Noll, Krehbiel, and others. The authors make a solid case for the utility of the public choice approach to legislative decision making; their volume will introduce students and general readers to this literature, which has achieved much currency in scholarly journals.

While making their case for this viewpoint, the editors are broad-gauged enough to appreciate the legacy of other approaches, and to admit the limits of the rational choice paradigm. As they note, the approach rests on an assumption of stability—not a bad assumption for our system. Yet in periods of rapid change or closely contested policies, legislative decisions may be unstable and frequently reversed.

Institutions themselves are malleable products of policy struggles. Those of us who watch Congress closely are forever reminded of the pliability of rules and structures, and the ingenious ways they can be bent or breached to serve the purposes of the moment. Bills may be passed without committee deliberation or even referral; jurisdictions are often trespassed; parliamentary rules are ignored or waived, even constitutional provisions circumvented (for example, that revenue bills originate in the House). Any calculations based on an immutable set of procedural "givens" is bound to distort reality. Although institutional arrangements are inertial (p. 316), they are constantly in motion.

Policy choices and institutional arrangements influence one another. True, institutions are bequeathed each generation of policymakers, quite apart from the policy agendas that may emerge. These institutional arrangements surely shape the way "problems" are interpreted and "solutions" chosen. But changing agendas push and stretch institutional structures, sometimes altering them profoundly even when outward forms remain intact. This is the lesson of Lowi and others: that policy type affects the way policy is handled.

McCubbins and Sullivan have produced an attractive showcase for the "new institutionalism." For the behavioralists' interpersonal equilibrium, however, has this new formalism substituted a stasis of institutional "rules"? Each approach has important, powerful things to tell us about the institution and its policies. Yet each leaves key questions unanswered.

ROGER H. DAVIDSON

University of Maryland

Exploring Long Cycles. Edited by George Modelski (Boulder: Lynne Rienner, 1987. x, 277p. $30.00).

The appearance of a collection of papers by Modelski and students provides an opportunity to assess the project associated with his name and studying the cyclical rise and fall of the great powers since the sixteenth century. There wasn't much data in these articles, and since the original Modelski piece in 1978, there does not appear to have been much concrete empirical research on documenting the actual periodicities of these purported cycles, save for the work of William Thompson and Karen Rasler. Simply put, Thompson and Rasler deserve more space and attention in this volume and more credit in general for continually trying to gather data and test ideas about the existence of long waves or cycles. In a very real sense they have been carrying this tradition with their continued high-quality journal articles, while Modelski seems content to repeat the initial observation of the original 1978 article as if there was no more research to be done.

The actual workings of cycles, let alone their very presence, needs research—real data about the world and not just the assumption that it exists. The recent work of Joshua Goldstein comes immediately to mind. In economics, where long statistical series are much more available, there is still great debate about the presence or absence of long swings, a skepticism that could shed some light and new thinking on the seemingly fixed Modelski scheme. Concerning data, there is a forthcoming book by Thompson and Modelski containing measures of sea power over these centuries, and it is anxiously awaited by all interested in the long wave, or cycle, phenomena.

There also needs to be more general reading in this tradition. There is simply too much self-reference among the annointed. Given the growing literatures in sociology and economics on long-wave phenomena, encountering other literatures would be most helpful. I think here specifically of the work of David Gordon, Edwards and Reich, G. Mensch, the Braudel scholars around Immanuel Wallerstein, Paul Kennedy, Andre Gunder Frank, and others. This would be particularly helpful because these authors have different dynamics in mind and the present system dynamics model favored by Modelski contains so much Parsonian gibberish that some new approaches would be welcome.

Following that point, something needs to be said about the prohegemonic sentiment that is involved in conceptualizing the dominance of a single state as a response to the global demand for "world leadership." This approach simply ignores the tremendous exercise of force in maintaining a hegemony and the differential economic benefits that flow to the hegemon. Modelski's assertion that "global war has played [the role] of the regular election process" (p. 126)—deciding, I suppose, the next hegemon—goes way beyond simply a benign view of the clearly crisis-prone nature of the international system. Given the presence of other ideas about war and the role of the hegemon it would certainly help the Modelski project to consider other implications of world dominance than simply that of leadership meeting a world demand for international order. There is a deep pathology to international life, nicely identified by Kenneth Boulding, that is simply masked in this functionalism.

ALBERT BERGESEN

University of Arizona

Politics in the Semi-Periphery: Early Parliamentarism and Late Industrialisation in the Balkans and Latin America. By Nicos P. Mouzelis (London: Macmillan, 1986. xix, 284p. $32.50).

This book analyzes the political trajectory of Argentina, Chile, Greece, and—secondarily—Bulgaria and Yugoslavia over the course of a century. It treats these countries, especially the first three, as a distinct unit, whose overarching similarity lies in the fact that they established parliamentary—albeit oligarchic—rule in the nineteenth century despite being in the at-best-incipiently industrialized semiperiphery of the world capitalist system.

The author notes that there have been three phases in their political evolution, each one of which is different from parallel developments in either the central or peripheral capitalist nations. The first, that of oligarchic parliamentarism, was characterized by a relatively strong state, a weak civil society, given the dearth of associational life (itself a product of the absence of *corps intermé diares* under colonial rule), and a paucity of popular political participation and mobilization, as the masses were either excluded altogether or subjected to clientelistic arrangements. This phase was relatively stable, given the fact that the societies were mostly rural and agrarian.

During the second phase, roughly from the end of World War I to the 1950s, the polity is changed by an increase in middle class and popular mobilization, both partly the result of growing industrialization and urbanization. Unlike similar developments in the developed West, however, in the semiperiphery the popular sectors are not able to achieve the formation of autonomous political and union organizations, which would have led to their "integration." Rather, they are "incorporated-excluded" (not "integrated") into the polity by a more widespread recourse either to clientelism, which occurs in the less urbanized and industrialized of these semiperipheral countries, or to populism, which is a phenomenon of the more urbanized and developed ones. In either case, the broad masses are still subjected to extensive controls by elites.

In the third phase, the popular sectors further increase their mobilization and demands, thereby forcing the limits of the "incorporation-exclusion" institutions of social and political control. At this point, the military becomes an increasingly important political actor, and it finally opts for taking over the government in order to install an authoritarian regime that will once again restrict popular political participation.

While much of the book discusses the political histories of the chosen countries (especially Argentina, Chile, and Greece) in order to illustrate these phases, it also contains theoretical analysis of the concepts of populism and clientelism and of the shortcomings of various conceptual approaches, particularly the Marxist, for the analysis of states and politics. I thought these latter arguments, which comprise the bulk of chapter 4, were the best in the book although they are not particularly novel; Mouzelis calls for an integration of Marxist and Weberian approaches, generating a class as well as an organizational and state-focused perspective.

The author is to be commended for attempting to do a broad-ranging comparison of cases drawn from different areas. However, I did not find this book's analysis particularly convincing. Firstly, any good comparative analysis must begin by noting the universe of cases to which the question being posed applies (the question in this work refers to the characteristics of the political evolution since the late nineteenth century of semiperipheral countries with early parliamentary political institutions). And yet the roster of cases that correspond to this generic type includes a good many more examples, such as Ireland, Portugal, Spain, Colombia, Uruguay, Peru, Brazil, and others, which are hardly if at all mentioned in the book. The author would have to explain more carefully his selection of cases or include more of them, in order to make a stronger argument for the fact that such countries do have a uniquely similar pattern of political development. Similarly, the author should explain more explicitly what the differences are between institutions in the semiperiphery and those in central countries, in order to buttress his arguments regarding the former.

Secondly, I was not impressed by the historical and analytical treatment of the Chilean case. I simply do not think that this case fits all that well the pattern Mouzelis puts it into. He is somewhat wary of his own treatment of Chile, given the fact that he frequently notes that Chile may constitute a "partial exception" to his generalizations, particularly regarding populism and the politicization of the military. The citations to the Chilean literature are not well done, and it is surprising that despite a lengthy discussion of the events leading to the military coup of 1973, there are no references to Arturo Valenzuela's well-known book on the subject. Valenzuela explicitly deals with, and rejects, the thesis Mouzelis (and others, such as Henry Landsberger and Tim McDaniel, who are also not cited) advance, namely, that the Chilean coup resulted from an excessive degree of popular mobilization. Similarly, although some authors do make these arguments, it is nonetheless very debatable whether Chile made a transition out of an oligarchic parliamentary form of rule in the 1920s, whether its political system could be characterized as something less than a democracy, given what are in the text ill-specified "incorporative-excluding" institutions, and whether its military was as heavily involved in politics before 1970 as the text paints it. The Chilean case is, however, very important for the general argumentation of this book, since it is certainly one of the modal cases in Mouzeli's type of early parliamentarism in the semiperiphery. My impression is that the author was all too eager to force Chilean history into his categories; a more thorough understanding of *it* would probably have led him to modify *them.*

Finally, I missed seeing any references to the fact that the author is not alone in using the term *incorporation.* David Collier and Ruth Collier's work on labor employs the same expression, and Alfred Stepan's work on Peru uses the very similar term, *exclusionary corporatism.* A longer treatment of the specific characteristics of the "incorporative-excluding" institutions Mouzelis mentions, and of the differences—if any—between his conception of them and those of others would have been a welcome addition to this book.

And yet, despite these misgivings, this is a provocative piece that may stimulate further comparative treatments and discussions of political development in these countries, which have been relatively neglected in the comparative politics literature.

J. SAMUEL VALENZUELA

University of Notre Dame

The Welfare State East and West. Edited by Richard Rose and Rei Shiratori (New York: Oxford University Press, 1986. 209p. $29.95).

The study of advanced industrial societies has been invigorated in recent years by sustained comparative investigations of the modern welfare state. Inquiry into the origins and trajectories of social welfare policies and institutions has become one of the most lively and theoretically interesting areas in comparative studies and historical sociology. At the same time, theoretical advances in this field have been matched by a new awareness of the fragile and shifting socioeconomic foundations of the welfare state. We have begun to understand the social forces and historical structures upon which the welfare state is built just as those forces and structures have endangered and rendered problematic the public provision of social welfare.

Amidst these developments, this edited collection of empirical country studies provides an important and distinctive contribution to our understanding of the modern welfare state. This contribution involves both a broadening and a deepening of our focus. The volume broadens the research agenda by bringing Japan into the comparative framework; hence the attention to the welfare state, East and West. In many respects, Japan represents the first non-Western industrial society to emerge on the world stage. As such, its distinctive patterns of industrial capitalism and politics challenge our European and U.S. models of political economy and relationships of state and society.

The juxtaposition in this volume of the Japanese experience with those of the United States and European countries also serves to deepen the conceptions of social welfare, and this is the volume's most original contribution. The authors seek to probe not just differences in the institutions and policy trajectories of social welfare, but also the larger social setting and cultural traditions that define national perceptions of welfare. In this sense, the editors are less concerned with presenting new theoretical explanations for welfare state development and more concerned with exploring national differences in what in fact constitutes social welfare. This leads Richard Rose to make a series of useful distinctions between public and private realms of welfare provision. When a "societywide approach to welfare" (p. 16) is adopted, the focus shifts to the role played by households and communities as well as that played by the state. Our theoretical attention must also shift to encompass cultural variations in the role of state, society, family, and the individual.

The strength of the volume is in the light it casts on national variations in the location and style of social welfare provision. The volume is less successful in advancing our understanding of the origins of these differences. The volume is helpful in revealing the divergent national experiences that need explanation; it is less helpful in advancing explanations of those differences. For those searching for concepts of the welfare state and for broad comparative sketches of alternative routes in welfare state development, this volume will be very illuminating. For those working in the explanatory trenches, the advice is to keep digging.

G. JOHN IKENBERRY

Princeton University

The Business of the Japanese State: Energy Markets in Comparative and Historical Perspective. By Richard J. Samuels (Ithaca: Cornell University Press, 1987. xii, 359p. $45.00, cloth; $14.95, paper).

This is a very good book on Japanese industrial policy, and on state intervention in markets generally. Samuels does everything right: he speaks to the key issues in the literature, systematically compares Japan to the relevant European examples, argues from solid historical research, and actually tests important hypotheses. The facts he presents simply demolish the hypothesis that the main engine of Japan's economic success was the "capitalist developmental state"; his interpretations call into question the usefulness of several theories of state intervention, and even of the very concept of "state."

The book is a "critical-case study" of the energy sector in Japan, surely as good a test of "state leadership" as can be imagined. Its heart is a chapter each on the modern political history of the coal (back 200 years), electric, oil, and alternative and nuclear energy industries in Japan, preceded by a chapter on parallel developments in Europe. No doubt specialists could find weak points in these accounts, but they are well documented and impressive in scope and detail. The major finding is that Japan is a unique "case of extensive state jurisdiction without a competitive commercial presence in the energy market place" (p. 257). Unlike Europe, where "national champion" state-owned corporations have been so common in the energy field, Japanese interventions have consistently been market-confirming, mostly in the form of funds and guarantees against risk. Moreover, the state rarely anticipated or caused market transformations but typically reacted after the fact.

That Japanese industrial policy is generally market-confirming and avoids direct controls has not been ignored by its more perceptive analysts, such as Chalmers Johnson (*MITI and the Japanese Economic Miracle* [1982]), but the usual explanation has been the wisdom, power, and skill of the economic bureaucrats—indirect methods as their strategy of choice. Samuels demonstrates that it is not for want of trying that the state does not participate directly. The Ministry of International Trade and Industry and its predecessors again and again tried to gain real control over energy markets, including the creation of national champions, and always failed. In not one of the 70 cases of market transformation described here was the state able to prevail over private interests; nearly always, it served them.

Does this book then revive the old image of Japan run by big business? Yes and no. Certainly it emphasizes the power of business actors—not just individual firms but also the critical and understudied political roles of various groupings of firms and, especially, of the banks. But business power is limited, both by internal competition among firms and groupings and by the countervailing power of other social interests (including labor in the postwar years) and of the state itself, which, while not fully autonomous, is nobody's captive.

Samuel's view thus generally fits into recent interpretations of Japanese politics as "patterned pluralism" and so forth, but more than others he stresses pervasive conflict, in the form of endless negotiation and renegotiation as changes in the economic, social, political, and international environments alter the resources, constraints, and opportunities of each actor. Over time, a norm of "reciprocal consent" emerges, under which "the state often helps structure market forces, but public/private negotiations invariably structure state and market choices alike" (p. 2). The politics of reciprocal consent can be found in Europe as well, but it is especially characteristic of Japan.

The main question for the comparative analysis is, Why no state ownership in Japan? and of course why something does *not* occur is difficult to explain. Samuel's research design focuses not on a single independent variable (e.g., What is the impact of industrial policy?) but on the dependent variable, market transformation (What causes changes in relations among producers and consumers?). In principle, he thus can balance state intervention against other causal factors: the strategies of private actors, outside forces, timing, and luck. In practice, Samuels proceeds mainly from the political science literature, evaluating six hypotheses that try to predict the extent and shape of state intervention in markets. These have to do with market structure, governmental centralization, developmental timing, openness to the international economy, and the nature of ruling coalitions. The results of testing these variables (nonquantitatively) among the national systems and of comparing Japanese subsectors and cases are not very promising for theory builders: each explanation is significant but sometimes in subtle or contradictory ways, and interaction often overwhelms the direct effects. Such hypotheses thus appear more useful as a checklist of factors for explicating particular cases than as a basis for a replicable theory. (Or perhaps a "process theory" rather than "variance theory" approach might work better.)

The hypothesis that works best is the breadth and stability of the ruling coalition. This conclusion rests on two sorts of evidence: market-disconfirming interventions in Europe have often been associated with shifts in relatively narrow ruling coalitions; and stable rights of participation for many actors appear to be a necessary condition for the reciprocal consent norm, which inhibits control by one actor. It is striking, incidentally, that patterns much like reciprocal consent appear in quite different Japanese settings: the conflict leading to "consensus" in villages found by the anthropologist Robert C. Marshall (*Collective Decision Making in Rural Japan* [1984]); the intricate relationships among regional textile producers depicted by Ronald Dore (*Flexible Rigidities* [1986]); various accounts of company organization, the budgeting system, and even radical protest groups. Stability and inclusion are key factors in all these fragmented systems of institutionalized conflict. Dare one suggest that a cultural explanation—albeit more sophisticated than the usual "harmony" model—might also be considered?

The still broader context of this book is the recent literature on "the state," and the discussion—while quite interesting—tends to reinforce a suspicion that U.S. political scientists not weaned on Hegel usually find this concept more distracting than helpful. The problem is not just that the Japanese government is so penetrated by private interests; in two-thirds of Samuel's cases, divisions *within* the state affected outcomes significantly, indicating that agencies, groups, and subgovernments may be a more productive level of analysis. They have interests and strategies, as do the private actors, while the "state" apparently does not.

Given that industrial policy is the hot topic in the Japan field these days and that Japan is cited as the exemplar of strong state leadership of the economy in so many comparative studies, this book is bound to provoke widespread debate. Samuels's facts and arguments are sure to stand up very well.

JOHN CREIGHTON CAMPBELL

University of Michigan

Erratum

Jacobsohn, Gary J. *The Supreme Court and the Decline of Constitutional Aspiration.* Reviewed by Stanley Brubaker (September 1987, 997-98). Page 998, col. 2, line 27 should read "think, of the nobility of the constitution, but."

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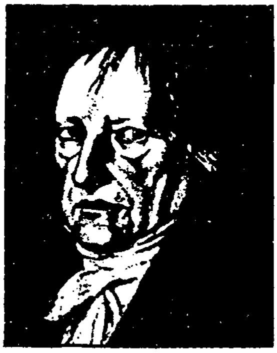

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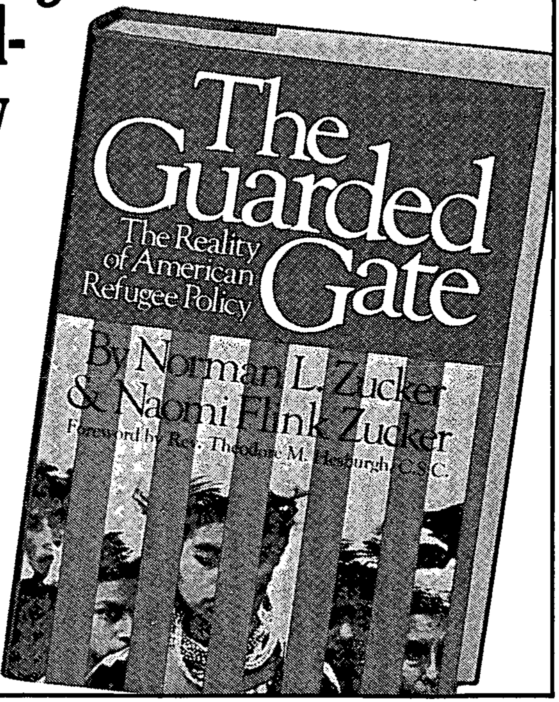
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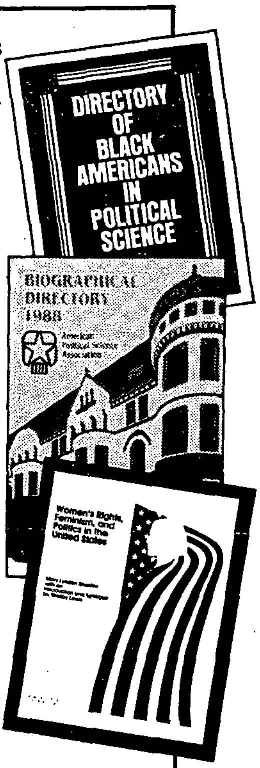
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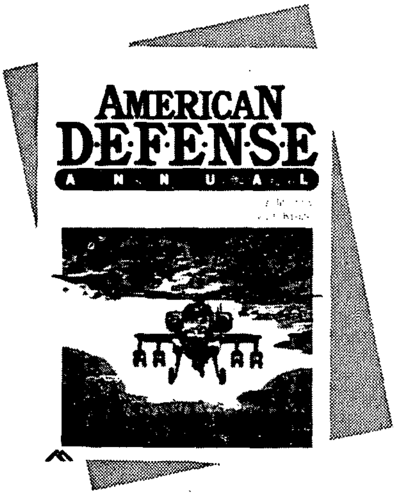

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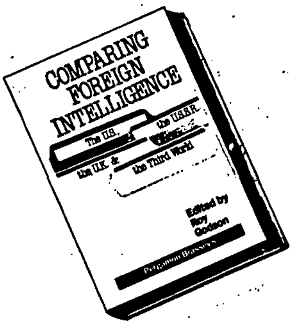
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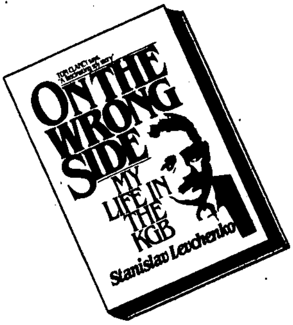
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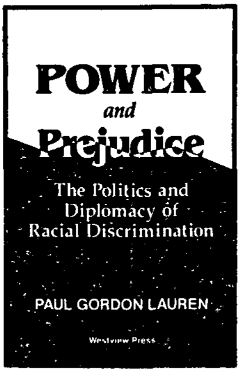

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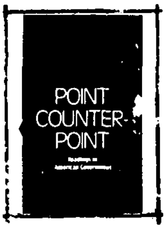

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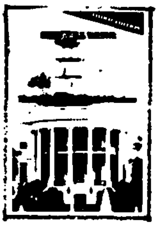

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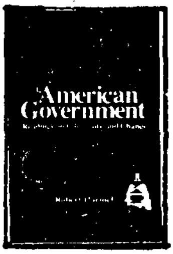

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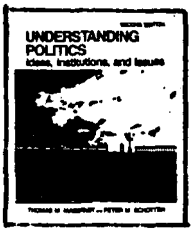

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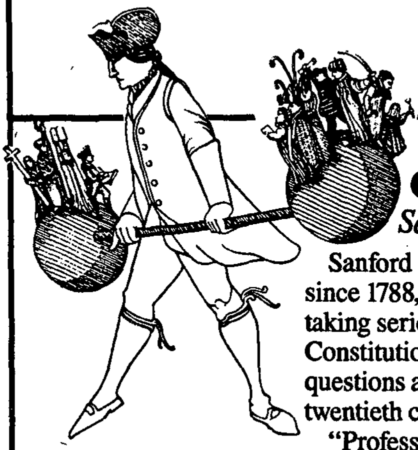

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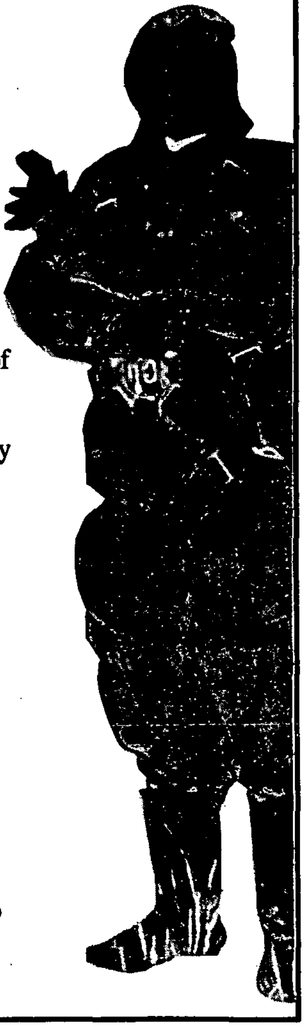

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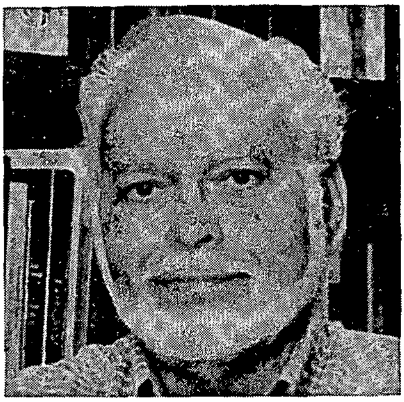

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